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CONTENTS OF No. LXXV

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

JULY, 1866.

Analytical and Critical Reviews.

PAGE

REV. I.—Principles and Practice of Medical Jurisprudence. By ALFRED SWAINE TAYLOR, M.D., F.R.S. 1

On Poisons, in Relation to Medical Jurisprudence and Medicine. By Dr. TAYLOR . . . . ib.

1. Handbuch der Giftlehre. Von Dr. A. W. M. VAN HASSELT, nac der Zweiten Auflage aus dem Holländischen bearbeitet von Dr J. B. HENKEL, zu Tübingen . . . . ib.

Handbook of Toxicology. By Dr. A. W. M. VAN HASSELT, translated from the Dutch of the 2nd Edition by Dr. J. B. HENKEL . . ib.

2. Handbuch der Toxicologie im Anschlusse an die Zweite Auflage von A. W. M. VAN HASSELT'S Hanleidung tot de Vergiftleer von Dr. Th. und Dr. A. HUSEMANN . . . . ib.

Handbook of Toxicology, founded on the Second Edition of Van Hasselt. By Drs. HUSEMANN . . . . ib.

3. Handbuch der Speciellen Pathologie und Therapie. Redigirt von R. VIRCHOW. Zweiter Band, Erste Abtheilung. Intoxicationen, bearbeitet von Prof. FALCK . . . . ib.

Handbook of Special Pathology and Therapeutics. Edited by R. VIRCHOW. 2nd vol., 1st Part. Intoxications, by Prof. FALCK . . ib.

4. Die Gerichtliche Chemie fur Gerichtsärzte und Juristen. Bearbeitet von Dr. F. C. SCHNEIDER 2

Medico-legal Chemistry. By Dr. F. C. SCHNEIDER . . . . ib.

5. Anleitung zur Ausmittelung der Gifte von. Prof. FR. JUL. OTTO, zweite durch einen Nachtrag vermehrte Auflage . . . . ib.

A Manual of the Detection of Poisons. By Dr. F. J. OTTO. Translated, with Notes and Additions, by WILLIAM ELDERHORST, M.D. . . ib.

6. Lehrbuch der Gerichtlichen Medizin. Von Dr. ADOLF SCHAUENSTEIN ib.

Text-book of Forensic Medicine. By Dr. A. SCHAUENSTEIN . . . ib.

7. Manuel Complet de Médecine Légale. Par J. BRIAND et E. CHAUDÉ ib.

Complete Manual of Legal Medicine. By J. BRIAND and E. CHAUDÉ . ib.

8. A Handbook of the Practice of Forensic Medicine. By J. L. CASPER ib.

9. Annales d'Hygiène, Henle's Zeitschrift, Casper's Vierteljahrschrift, Canstatt's Jahresberichte, &c. . . . . ib.

REV. II.—On the Cure of Club-foot without Cutting Tendons, and on certain New Methods of Treating other Deformities. By RICHARD BARWELL, F.R.C.S., &c. 23

PAGE

Rev. III.—1. Traité Théorique et Pratique des Maladies de l'Oreille et des Organes de l'Audition. Par le Docteur J. P. Bonnafont . . 29
A Theoretical and Practical Treatise on Diseases of the Ear and of the Organs of Hearing. By Dr. J. P. Bonnafont . . . . ib.
2. The Diseases of the Ear, their Diagnosis and Treatment; a Text Book of Aural Surgery, in the form of Academical Lectures. By Dr. Anton von Troeltsch (translated from the German, and edited by Dr. St. John Roosa) . . . . ib.
3. A Vindication of the Present State of Aural Surgery. By a Member of the New Sydenham Society . . . . ib.
4. On a New Method of applying Remedial Agents to the Cavity of the Tympanum. By Dr. Edward Bishop . . . . . ib.
5. Ohrenkrankheiten und Ohrenärzte in England und Deutschland; Ein Nachtrag zur Ohrenheilkunde der Gegenwart. Von Dr. W. Kramer ib.
Diseases of the Ear and Aurists in England and Germany; a Supplement to the Aural Surgery of the present day. By Dr. W. Kramer . ib.
Rev. IV.—Surgical Experiences: the Substance of Clinical Lectures. By Samuel Solly, F.R.S., &c. 37
Rev. V.—A System of Medicine. Edited by J. Russell Reynolds, M.D., F.R.C.P. Eng. 43
Rev. VI.—1. Etudes Ophthalmologiques. Traité Théorique et Pratique des Maladies des Yeux. Par L. Wecker, Docteur en Médecine des Facultés de Wurzbourg et de Paris, &c. 62
Ophthalmological Studies. A Theoretical and Practical Treatise on Diseases of the Eyes. By Dr. Wecker . . . . . ib.
2. A Treatise on the Principles and Practice of Ophthalmic Medicine and Surgery. By T. Wharton Jones, F.R.S., &c. . . . . ib.
3. A Guide to the Practical Study of Diseases of the Eye; with an Outline of their Medical and Operative Treatment. By James Dixon, F.R.C.S., &c. . . . . ib.
4. A Handy Book of Ophthalmic Surgery, for the use of Students and Practitioners. By J. Z. Lawrence, M.B., F.R.C.S., &c. . . . 63
5. Étude Ophthalmoscopique sur les altérations du Nerf Optique, et sur les Maladies Cérébrales dont elles dépendent. Par Xavier Galezowski, Docteur en Médecine de la Faculté de Paris, &c. . ib.
The Ophthalmoscopic Investigation of changes in the Optic Nerve, and of the Cerebral Maladies on which they depend. By Dr. Xavier Galezowski . . . . ib.
6. Lectures on Diseases of the Eye; referring principally to those affections requiring the aid of the Ophthalmoscope for their diagnosis. By N. C. Macnamara, Surgeon to the Calcutta Ophthalmic Hosp., &c. ib.
7. Die Physiologische Optik. Eine Darstellung der Gesetze des Auges, und der Sinnesthätigkeiten überhaupt. Von Dr. Hermann Scheffler . . . . ib.
Physiological Optics. An Exposition of the Laws Governing the Eyes, and the Senses generally. By Dr. Hermann Scheffler . . . ib.
8. The Optical Defects of the Eye and their Consequences, Asthenopia and Strabismus. By J. Z. Lawrence, M.B., F.R.C.S., &c. . . ib.
9. On the Means employed for Correcting the Inverted Image on the Retina of the Eye. By Joseph Swan . . . . . ib.
Rev. VII.—Bidrag til Bedömmelsen af Födemidlernes Næringsværdi. Af Dr. med. P. L. Panum, Professor i Physiologien ved Kjöbenhavns Universitet 83
A Contribution to the Estimation of the Nutritive Value of Foods. By P. L. Panum, M.D., &c. . . . . ib.
Rev. VIII.—1. The Elements of Prognosis in Consumption, with Indications for the Prevention and Treatment. By James Edward Pollock, M.D. 93
2. On the Temperature of the Body as a means of Diagnosis in Phthisis and Tuberculosis. By Sidney Ringer, M.D. . . . . ib.

PAGE
Rev. IX.—1. Saint Bartholomew's Hospital Reports. Edited by Dr. Edwards and Mr. Callender. Vol. I. 103
2. Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital. Vol. II, 1865 . . . . . ib.
Rev. X.—1. Report on Cholera. By Drs. Baly and Gull . . . 129
2. Notes on Cholera. Geo. Johnson, M.D., F.R.C.P., &c. . . ib.
3. The Science and Practice of Medicine. By Wm. Aitken, M.D. . ib.
4. A Theoretical Inquiry into the Physical Cause of Epidemic Diseases. By Alex. Hamilton Howe, M.D. Lond. . . . . . ib.
5. Supplement to the Twenty-fifth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England, 1864 . . ib.
6. Reminiscences of the Cholera at St. George's Hospital in 1849 and 1854. J. W. Ogle, M.D., F.R.C.P., &c. . . . . . ib.
7. On the Cholera in the Hospital of St. Antoine, Paris. E. Mesnet, M.D. ib.
8. Handbook for Yellow Fever, &c., and a Brief History of Pestilential Cholera, and a method of Cure. T. Anderson, M.D., F.R.C.S.E., &c. ib.
9. Clinical Lectures on the Practice of Medicine. By the late R. J. Graves, M.D., F.R.S. Reprinted from second edition, by the late J. M. Neligan, M.D. . . . . ib.
10. Lectures on the Principles and Practice of Physic. By Thos. Watson, M.D., &c. . . . . ib.
11. Memorandum of the Metropolitan Medical Officers of Health, as a base of common action in cases of Localities visited by Cholera . ib.
12. A System of Medicine. Edited by J. Russell Reynolds, M.D., F.R.C.P. Lond., &c. Art. "Epidemic Cholera," by Edward Goodeve, M.B., &c. . . . . ib.
13. Researches on Disease in India. C. Morehead, M.D., &c. . . 130
14. Dissertations on Malaria, Contagion, and Cholera, &c. By Wm. Aiton, M.D., &c. . . . . ib.
15. The Cholera at Finglas. C. F. Moore, M.D. . . . . . ib.
16. Apparent Causes of Fever and Cholera. C. F. Moore, M.D., F.R.E.S. ib.
17. La Choléra ou Typhus Indien Epidémie de 1865. Prophylaxie et Traitement. Par le Docteur Charles Pellarin . . . . ib.
18. Cholera: its Pathology, Diagnosis, and Treatment. By Wm. Story, L.K. and Q.C.P.I., F.R.C.S.E., &c. . . . . . . ib.
19. Cholera Prospects, &c. By Tilbury Fox, M.D. Lond., &c. . . ib.
20. De la Contagion dans les Maladies. Par M. le Docteur Stanski . ib.
21. Cholera at Vizagapatam in 1861. J. Donaldson, M.D. . . ib.
22. Le Choléra et le Congrès Sanitaire Diplomatique International. Par le Docteur J. P. Bonnafont . . . . ib.
23. Brief Remarks on Cholera, &c. By Robert J. Spitta, M.D. Lond. ib.

Bibliographical Record.

Art. I.—1. Philosophia Zoologica. Auctore J. Van der Hoeven . . 162
Philosophy of Zoology. By J. Van der Hoeven . . . . ib.
2. Catalogus Craniorum Diversarum Gentium, quæ collegit J. Van der Hoeven . . . . ib.
A Catalogue of the Crania of Various Races, collected by J. Van der Hoeven . . . . ib.
3. De Vera Anatomes Comparatæ Indole. Oratiuncula ad aperiendas Scholas, quam habuit die xxvi Septembris. J. Van der Hoeven . ib.
On the True Bearings of Comparative Anatomy. An Inaugural Address delivered at the Opening of the Academical Session on the 26th of September, by J. Van der Hoeven . . . . . ib.

PAGE

ART. II.—Sulla Causa specifica del Colera Asiatico, il suo processo patologico e la indicazione curativa che ne resulta. Memoria del Dott. FILIPPO PACINI 167

A Treatise on the specific Cause of Cholera, its Pathology and Cure. By Dr. FILIPPO PACINI . . . . ib.

ART. III.—On the Rapidity of Thought and of the Determination of the Will: A Preliminary Communication by Dr. F. C. DONDERS. Translated from the 'Nederlandsch Archief voor Genees en Natuurkunde.' By W. D. MOORE, M.D. Dub., M.R.I.A. 168

ART. IV.—Some Effects of the Climate of Italy. By THOMAS KING CHAMBERS, M.D., &c. 170

ART. V.—Observations on Meat (butchers' meat) in relation to the Changes to which it is liable under different circumstances. By JOHN DAVY, M.D., F.R.S., &c. 173

ART. VI.—On the existence in the Textures of Animals of a Fluorescent Substance closely resembling Quinine. By Dr. HENRY BENCE JONES, A.M., F.R.S., &c. 174

Original Communications.

ART. I.—Observations on (so-called) Nervous Apoplexy, on Congestion of the Brain and Serous Effusion. By W. BOYD MUSHET, M.B., M.R.C.P. 177

ART. II.—On the Diagnosis of the forms of Bright's Disease. By GRAINGER STEWART, M.D., F.R.C.P.E., F.R.S.E.; Pathologist and Extra Physician to the Royal Infirmary; Lecturer on General Pathology, Surgeons' Hall, Edinburgh 196

ART. III.—Case of Excision of a part of the Spinal Accessory Nerve for Spasmodic Wry Neck. By CAMPBELL DE MORGAN, F.R.S., Surgeon to the Middlesex Hospital, Examiner in Surgery to the Royal College of Physicians 218

ART. IV.—On the Non-identity of the Parasites met with in Favus, Tinea Tonsurans, and Pityriasis Versicolor, including proofs derived from the occurrence of these diseases amongst the lower animals, and their transmission from them to man. By Dr. McCALL ANDERSON, Lecturer on Practice of Medicine in Anderson's University, Physician to the Dispensary for Skin Diseases, &c. 225

ART. V.—On some of the Hospitals of Northern Germany and the adjacent Countries, with Remarks on Hospital Construction, and on the Plans to be adopted in the new St. Thomas's Hospital 236

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

Chronicle of Micrology. By J. F STREATFEILD, F.R.C.S., &c. . . 256

Quarterly Report on Surgery. By JOHN CHATTO, Esq., M.R.C.S.E.. . 268

Quarterly Report on Midwifery. By ROBERT BARNES, M.D. Lond., &c. . 277

BOOKS RECEIVED FOR REVIEW 288

CONTENTS OF No. LXXVI

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1866

Analytical and Critical Reviews.

PAGE

Rev. I.—1. Bidrag til Kundskab om Echinococcernes Udvikling, navnlig om Doettreblæredannelsen. Af Vald. Rasmussen, Prosector i patholog. Anatomi 285

A Contribution to our Knowledge of the Development of Echinococci, especially with reference to the Formation of Daughter-cells. By Vald. Rasmussen, Prosector in Pathological Anatomy . . ib.

2. Bidrag til Kundskaben om Echinococcernes Udvikling hos Mennesket, deres almindelige Pathologi og pathologiske Anatomi. Afhandling for Doctorgraden i Medicinen af Vald. Rasmussen, Prosector ved Kommunehospitalet . . . . ib.

A Contribution to our Knowledge of the Development of Echinococci in Man, their general Pathology and pathological Anatomy. A Thesis for the degree of Doctor in Medicine. By Vald. Rasmussen, Prosector to the Municipal Hospital . . . . . . ib.

3. Helminthologiske Undersögelser i Danmark og paa Island med særligt Hensyn til Blæreormlidelserne paa Island. Af Dr. med. H. Krabbe . ib.

Helminthological Investigations in Denmark and Iceland, with special reference to Hydatid Affections in Iceland. By H. Krabbe, M.D. . ib.

Rev. II.—A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits; illustrated by numerous Cases and Engravings. By W. Roberts, M.D., &c. 301

Rev. III.—A Proposal, in order to Diminish the Progress of the Distemper among Horned Cattle, supported by Facts. By Malcolm Flemming 307

An Essay on the Nature, Cause, and Cure of the Contagious Distemper among Horned Cattle in these Kingdoms. By Peter Layard . ib.

The Three Reports of the Commissioners appointed to inquire into the Origin and Nature, &c., of the Cattle Plague, with Appendices . ib.

The Cattle Plague; with Official Reports of the International Veterinary Congresses held in Hamburg, 1863, and in Vienna, 1865. By John Gamgee, Principal of the Albert Veterinary College, London . ib.

On the Cattle Plague, or Contagious Typhus in Horned Cattle: its History, Description, and Treatment. By H. Bourguignon, Doctor of the Faculté de Paris, &c. . . . . ib.

Rev. IV.—Lectures on Mental Diseases. By W. H. O. Sankey, M.D., &c. . 346

Rev. V.—The Harveian Oration, 1865. By Henry W. Acland, M.D., F.R.S., &c. 353

Rev. VI.—On Diseases of the Veins, Hæmorrhoidal Tumours, and other Affections of the Rectum. By Henry Lee, F.R.C.S., &c. . . 363

PAGE

Rev. VII.—1. Étude sur les Hôpitaux considérés sous le rapport de leur Construction, &c. Par Armand Husson 370

Hospitals considered with reference to their Construction, &c. By Armand Husson . . . . ib.

2. Rapport sur les Hôpitaux Civils de la Ville de Londres, à point de vue de la Comparaison de ces Établissements avec les Hôpitaux de la Ville de Paris. Par MM. Blondel et Ser . . . . . ib.

Report on the Civil Hospitals of London as compared with those of Paris. By MM. Blondel and Ser 371

3. Notice sur l'Hôpital Saint-Jean. Étude sur la meilleure Manière de Construire et d'Organiser un Hôpital de Malades. Par André Uytterhoeven . . . . ib.

Notice of the Hospital of St. John. On the best Mode of Constructing and Organising an Hospital for the Sick. By Andrew Uytterhoeven . . . . ib.

4. Discussion sur l'Hygiène et la Salubrité des Hôpitaux, à la Société de Chirurgie de Paris . . . . ib.

Discussion on the Hygiene and Salubrity of Hospitals, at the Surgical Society of Paris . . . . ib.

5. Notes on Hospitals. By Florence Nightingale . . . . ib.

6. On Hospitals. By Sir J. Ranald Martin, M.D. . . . . ib.

7. A Manual of Practical Hygiène. By Edmund Parkes, M.D. . ib.

8. Report by Dr. John S. Bristowe and Mr. T. Holmes on the Hospitals of the United Kingdom . . . . ib.

9. The 'Builder' (an Illustrated Weekly Magazine). Various articles on Hospital Construction . . . . ib.

10. Polizia degli Spedali. Scritta dal Dottor Giovanni Pozzi . . ib.

On Hospitals. By Dr. John Pozzi . . . . . . ib.

Rev. VIII.—Transactions of the Obstetrical Society, Vol. VII, 1866; with Classified Catalogue of the Library 397

Rev. IX.—A Practical Treatise on the Diseases of the Testis and of the Spermatic Cord and Scrotum. By T. B. Curling, F.R.S., Surgeon to the London Hospital, &c. 407

Rev. X.—1. Cholera in its Home, with a Sketch of the Pathology and Treatment of the Disease. By John Macpherson, M.D., &c. . . 417

2. Army Medical Department Statistical, Sanitary, and Medical Reports. —Statistical Report on the Health of the Navy for the Year 1862.—Report on the Epidemic Visitation of Cholera in 93rd Sutherlandshire Highlanders in 1862. By William Munro, M.D. . . . . ib.

3. An Essay to prove the Contagious Character of Malignant Cholera; with brief Instructions for its Prevention and Cure. By Bernard M. Byrne, M.D., U.S. Army . . . . ib.

4. On the Nature of Cholera, as a Guide to Treatment. Second Issue, with a New Section on Treatment. By William Sedgwick, M.R.C.S., Surgeon to the St. Marylebone Provident Dispensary, &c. . . ib.

Bibliographical Record.

Art. I.—On Disease of the Right Side of the Heart. By T. M. Daldy, M.D., late President of the Hunterian Society 429

Art. II.—Some Remarks on the Nature and Treatment of Pulsating Thyroïd Gland, with Exophthalmos ("Graves' Disease"). By William Moore, M.D., &c. 431

Art. III.—A Course of Practical Chemistry arranged for the use of Medical Students. By W. Odling, M.B., F.R.S. 432

PAGE

ART. IV.—On the Use of Carbolic Acid. From the 'Proceedings of the Odontological Society,' June 5th, 1865. By Mr. Woodhouse and Mr. Gibbons . . . 433

ART. V.—Report on the Causes of Death among the Assured in the Scottish Equitable Life Assurance Society from 1831 to 1864, being a period of thirty-three years. Submitted to the Board of Directors. By William Robertson, M.D. . . . ib.

ART. VI.—The Personal Responsibility of the Insane. By James F. Duncan, M.D., &c. . . . 434

ART. VII.—A Treatise on the Chronic Inflammation and Displacements of the Unimpregnated Uterus. By Wm. H. Byford, M.D. . . . ib.

2. The Practice of Medicine and Surgery applied to the Diseases and Accidents incidental to Women. By W. H. Byford . . . ib.

ART. VIII.—Elements of Physics, or Natural Philosophy. Written for general use, in non-technical language. By Neil Arnott, M.D., F.R.S., &c. . . . 436

ART. IX.—A Proposal for the institution of Degrees or Certificates of Qualification in State Medicine at the Universities of the United Kingdom. By H. W. Rumsey, F.R.C.S., Member of the General Medical Council, &c. . . . 438

ART. X.—Contributions to assist the Study of Ovarian Physiology and Pathology. By Charles G. Ritchie, M.D. . . . 439

ART. XI.—Des Maladies Mentales et des Asiles d'Aliénés. Par J. P. Falret, Médecin de l'Hospice de la Salpêtrière, &c. Avec un Plan de l'Asile d'Illenau . . . 441

ART. XII.—Orthopraxy. The Mechanical Treatment of Deformities, Debilities, and Deficiencies of the Human Frame. By Henry Heather Bigg . . . 444

ART. XIII.—Dictionary of Science, Literature, and Art. Edited by W. T. Brande, D.C.L., &c., and the Rev. G. W. Cox, M.A., &c. . . . 445

ART. XIV.—The United States' Sanitary Commission. A Sketch of its Purposes and Work. Compiled from Documents and Private Papers. Published by Permission . . . 447

ART. XV.—Report on the Physiological Action of the Nitrite of Amyl. By B. W. Richardson, M.A., M.D. . . . 450

ART. XVI.—On the Detection of Aconite by its Physiological Action; being Notes of Experiments made in connection with the Trial of Dr. E. W. Pritchard. By Frederick Penny, F.R.S.E., &c., and James Adams, M.D., F.F.P.S. . . . 452

2. On the Application of Physiological Tests for certain Organic Poisons, and especially Digitaline. By C. Hilton Fagge, M.D., and Thomas Stephenson, M.D. . . . ib.

ART. XVII.—What Food to Eat. By W. W. Ireland, M.D. (late of H.M. Bengal Army) . . . 456

ART. XVIII.—Sanitary Commission, No. 87. Preliminary Reports of the Operations of the United States' Sanitary Commission in North Carolina, March, 1865, and upon the Physical Condition of the Exchanged Prisoners lately received at Wilmington, North Carolina 460

2. Narrative of Privations and Sufferings of United States' Officers and Soldiers while Prisoners of War in the hands of the Rebel Authorities. Being the Report of a Commission of Inquiry, appointed by the United States' Sanitary Commission. With an Appendix containing the Testimony . . . ib.

ART. XIX.—The Anthropological Treatises of Johann Friedrich Blumenbach, late Professor at Göttingen, &c. With Memoirs of him by Marx and Flourens, and an Account of his Anthropological Museum, by Professor M. Wagner, and the Inaugural Dissertation of John Hunter, M.D., on the Varieties of Man. Translated and Edited from the Latin, German, and French originals, by Thomas Bendyshe, M.A., V.P.A.S.L., &c. . . . 465

PAGE

ART. XX.—Defectus Uteri et Vaginæ. Ved Dr. F. C. FAYE, Professor i Accouchement ved Norges Universitet, Overlæge ved Födselsstiftelsen og Börnehospitalet i Christiania 469

On Absence of the Uterus and Vagina. By Dr. F. C. FAYE, Professor of Midwifery in the University of Norway, First Physician to the Lying-in Institution and Hospital for Children in Christiania . ib.

Inflammatoriske, hypertrophiske og fibröse Svulsttilstande af Livmoderhalsen. Ved Dr. F. C. FAYE . . . . ib.

On Inflammatory, Hypertrophic, and Fibrous Tumours of the Cervix Uteri. By Dr. F. C. FAYE . . . . ib.

ART. XXI.—Systems of Admissions at Hospitals.—A Plea for Reform: being a selection from a series of articles reprinted (by permission) from the 'Birmingham Daily Post.' With a Preface 471

ART XXII.—1. Chloroform; its Action and Administration. A Handbook. By ARTHUR ERNEST SANSOM, M.D., &c. 474

2. On the safe Abolition of Pain in Labour and Surgical Operations by Anæsthesia with mixed Vapour. By ROBERT ELLIS, Surgeon-Accoucheur, &c. . . . . ib.

ART. XXIII.—Contributions to Dermatology. By Dr. MCCALL ANDERSON. On the Non-identity of the Parasites met with in Favus, Tinea Tonsurans, and Pityriasis versicolor 476

ART. XXIV.—Gout and Rheumatism in relation to Diseases of the Heart. By A. W. BARCLAY, M.D., &c. 477

Original Communications.

ART. I.—Essay on the Medicine of the Greeks. By T. CLIFFORD ALLBUTT, B.A., M.B. Cantab., &c. 483

ART. II.—On the Reduction of the Subcoracoid Dislocation of the Humerus by Manipulation. By ALEXANDER GORDON, M.D., L.R.C.S. Ed., &c. 498

ART. III.—On the Influence of Age in Hereditary Disease. By WILLIAM SEDGWICK 501

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

Half-Yearly Report on Materia Medica and Therapeutics. By ROBERT HUNTER SEMPLE, M.D., &c. 524

Half-Yearly Report on Pathology and Principles and Practice of Medicine. By FRANCIS C. WEBB, M.D., F.L.S., &c. 539

Contributions to Medical Literary History:
Adversaria Medico-Philologica. By W. A. GREENHILL, M.D. Oxon. . 551

'Dixon's Guide to the Practical Study of Diseases of the Eye,' &c. . . 560

BOOKS RECEIVED FOR REVIEW 561

TITLE, CONTENTS, INDEX, &c.

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MEDICO-CHIRURGICAL REVIEW.

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PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

Principles and Practice of Medical Jurisprudence. By ALFRED SWAINE TAYLOR, M.D., F.R.S. *London:* Churchill. 1865. 8vo. 1186 pp.

On Poisons, in Relation to Medical Jurisprudence and Medicine. By Dr. TAYLOR. 2nd Edit. *London.* 1859. pp. 863.

1. *Handbuch der Giftlehre.* Von Dr. A. W. M. VAN HASSELT, nach der Zweiten Auflage aus dem Holländischen bearbeitet von Dr. J. B. HENKEL, zu Tübingen. *Braunschweig:* Vieweg und Sohn. 1862.

Handbook of Toxicology. By Dr. A. W. M. VAN HASSELT, translated from the Dutch of the 2nd Edition by Dr. J. B. HENKEL. *Brunswick.* 2 vols.

2. *Handbuch der Toxicologie im Anschlusse an die Zweite Auflage* von A. W. M. VAN HASSELTS Hanleidung tot de Vergiftleer von Dr. Th. und Dr. A. HUSEMANN. *Berlin:* Reimer. 1862

Handbook of Toxicology, founded on the Second Edition of Van Hasselt. By DRS. HUSEMANN. 2 vols. pp. 978.

3. *Handbuch der Speciellen Pathologie und Therapie.* Redigirt von R. VIRCHOW. Zweiter band, Erste Abtheilung. Intoxicationen bearbeitet von Prof. Falck. *Erlangen:* Enke. 1855.

Handbook of Special Pathology and Therapeutics. Edited by R. VIRCHOW. 2nd vol., 1st Part. Intoxications by Prof. FALCK. pp. 336.

4. *Die Gerichtliche Chemie fur Gerichtsärzte und Juristen.* Bearbeitet von Dr. F. C. SCHNEIDER. *Wien:* Braumüller. 1852.

Medico-legal Chemistry. By Dr. F. C. SCHNEIDER. *Vienna.* 1852.

5. *Anleitung zur Ausmittelung der Gifte* von Prof. FR. JUL. OTTO, zweite durch einen Nachtrag vermehrte Auflage. *Braunschweig:* Vieweg und Sohn.

A Manual of the Detection of Poisons. By Dr. F. J. OTTO. Translated, with Notes and Additions, by WILLIAM ELDERHORST, M.D. *New York:* Baillière. 1857. pp. 178.

6. *Lehrbuch der Gerichtlichen Medizin.* Von Dr. ADOLF SCHAUENSTEIN. *Wien:* Braumüller. 1862.

Text-Book of Forensic Medicine. By Dr. A. SCHAUENSTEIN. *Vienna.* pp. 512.

7. *Manuel Complet de Médecine Légale.* Par J. BRIAND et E. CHAUDÉ. 7ieme Edition. *Paris:* Baillière. 1863.

Complete Manual of Legal Medicine. By J. BRIAND and E. CHAUDÉ. 7th Edition. *Paris:* Baillière. pp. 1048.

8. *A Handbook of the Practice of Forensic Medicine.* By J. L. CASPER. Translated for the New Sydenham Society. *London,* 1861, et sq. 4 vols. pp. 1439.

9. *Annales d'Hygiène, Henle's Zeitschrift, Casper's Vierteljahrschrift, Canstatt's Jahresberichte, &c.*

FROM time to time, as science advances, we do well to follow the example of careful merchants, who at stated intervals sum up their gains and losses, in order to ascertain the exact position of their affairs. Fortunately for us, we have no real losses to record, unless we count as such the departure of those great men who have spent their lives in the service of science, for clearing away the mists that rendered some fact doubtful, or even the ascertaining of its falsehood, must be viewed as actual progress.

The appearance of Dr. Taylor's 'Principles and Practice of Medical Jurisprudence,' the most elaborate work in the English language that has yet appeared on the subject, not even excepting that of the Brothers Beck, may well serve as a signal for one of those periodical stock-takings. In reviewing such a work as that to which we have just referred, due allowance must be made for the difficulties attached to the position of a systematic writer. His information must be culled from very varied sources, much care must be taken to ascertain the character of that which he accepts, subjects to which he has directed special attention must be kept in the place to which they properly belong, however subordinate that may be, and the whole brought abreast of the most recent investigations. Altogether, we must congratulate Dr. Taylor on the success which has followed his labours in this direction; and though there may be some things we should have liked to see in his work which we do not find there, as well as some others in which we can hardly agree with him, still, we know only too well how much easier it is to find fault than to amend, to cast down than to raise up. The position of the reviewer is also one of very great delicacy, liable as we all are to have our critical eyes blinded by those personal likings or antipathies of which it is so difficult to entirely divest ourselves; and this must be especially borne in mind in the case of a man occupying Dr. Taylor's position, who has been, it may be, too lavishly praised at one time, only to be, most certainly, too much blamed at another. In treating of the subject at large, it must also be borne in mind that the position of medical jurisprudence in England is very peculiar and far from satisfactory; there are not, as on the Continent or even in some parts of Scotland, men specially trained and set apart for conducting inquiries in any case calling for investigation; everything is left in the hands of the coroner, who, except he happen to be a medical man, often takes no heed of the manner in which a post-mortem examination has been made, or of the grounds on which the conclusions officially stated before him have been arrived at.

No man in England has probably done so much to elevate forensic medicine to the dignity of a science as Dr. Taylor, but the materials he has had to work with have, for the reasons just stated, too often been wanting in that accuracy and exactness which is so necessary when the opinions to be founded on them have to be stated in a court of law. Again and again, in reading his work, do we meet with such phrases as "communicated to me by Dr. — or Mr. —," without full particulars being given; in such instances we must either trust entirely to the judgment of the reporter or reject his report as useless, should we desire to found any opinion on it. Even in those cases specially reported for our medical journals it too often happens that the very point necessary to enable us to come to a decision, from a medical point of view, has either been overlooked or omitted from the report. Is it not a striking fact that in a country described by a recent French writer of authority as one which "appears to be, and ought to remain, privileged for toxicological studies," there is not a single journal devoted to medical jurisprudence, so that students of such subjects have to search through the whole range of our periodical literature for what will but too often prove a Dead Sea apple when acquired? So much is this the case that a familiarity with the French and German languages is indispensable to any one who seeks thorough knowledge in this department of medicine, for it is thither we must turn if we want to know how it progresses.

With these preliminary remarks we must content ourselves and turn to the subject which more immediately demands our attention. On the present occasion we shall limit ourselves to General Toxicology, with special reference to Dr. Taylor's recent work, reserving our remarks on the other departments of medical jurisprudence for one or more future articles. Our object, in this one, is to lay before our readers the advances which general toxicology has made in recent years, and, as far as is possible in a single article, to make them acquainted with its exact condition.

At the very outset of our inquiries we are met by the *questio vexata*—what is a poison? to which so many replies have been given, and which is as far as ever from being satisfactorily answered. In the works enumerated at the head of this article there are all sorts of definitions; some, like Falck, giving very elaborate ones, others following the example of Christison by evading the subject altogether. Bœcker denied the possibility of defining a poison at all, and in this he is followed by Schauenstein and Tardieu. These authors maintain that poisons are not substances having a positive existence, but are mere potentialities, which under certain circumstances are capable of acting in a particular, *i. e.* poisonous, manner. As an illustration of this dogma, Schauenstein gives a case, which occurred in his own practice, of a person who, for suicidal purposes, took a quantity of amygdalin, a substance which by itself is inert, but which, should it come in contact with emulsin, is resolved into prussic acid, &c. Now, it is very questionable if a case of poisoning where the victim had been dosed first with amygdalin and then with emulsin could come within the range of our statute, seeing that, as in some other countries, it is founded, not on the result produced, but on the cause of this result. The importance of obtaining a proper definition of the word "poison" is all the greater since in the witness-box the medical man is left to state what a poison really is, and whether a certain substance can be considered a poison. Abroad, this difficulty is avoided by the law defining a poison (as in France) to be "any substance capable of causing death," while the supreme Prussian tribunal recognise as poisonous "all substances which by virtue of their properties are capable of destroying health." But though these are satisfactory in a legal point of view, they are far from being so scientifically, nor is that recommended by Dr. Taylor, viz., that "a poison is a substance which, when absorbed into the blood, is capable of seriously affecting health or of destroying life," much better, seeing that it excludes all substances which act locally, and kill, it may be, from the constitutional disturbances occasioned by these local derangements. He has himself cited the corrosive poisons as exceptions to his definition. Such being the case, we are inclined to side with Tardieu, who rejects the idea of *a poison* altogether, and limits himself to that of *poisoning*, which he defines as "an accidental morbid state resulting from the special action of certain deleterious mineral and organic substances on the economy;" for, seeing that poisons can only be defined by the results which they produce, it is better to go at once to these results and to abide by them in our definitions. It is of no consequence whether, in death by strangulation, the fingers or a rope has been employed, the result is the same in both cases; so is it here, if death has been brought about by any substance administered for that purpose, it really matters not whether this substance be usually considered a poison or not.

A good deal of ingenuity has also been uselessly expended in attempting to distinguish between *medicines* and *poisons*, much stress being laid for this purpose on the dose in which they are administered; but we well know that medicinal doses of such substances as tartar emetic, mercury, or arsenic, continued for a protracted period will as infallibly produce symptoms of poisoning, and that too in its most intractable form, as a single large dose. Nor is this the only case, for we also know that medicinal doses of substances usually innocent may, under peculiar circumstances, produce the most alarming results, and a medicinal dose of arsenic or tartar emetic might prove as dangerous during gastritis as an enormous one when the stomach was healthy.

One of the greatest deficiencies of modern toxicology is the want of a proper classification of the substances included under the common designation of poisons. Here Dr. Taylor has done something to assist us, though he still, in principle, adheres to that modification of Foderé's classification adopted by Dr. Christison.

All sorts of schemes have been set forth for arranging the numerous substances known to possess poisonous properties into groups of a convenient size. In that adopted by Foderé, and which in a somewhat modified shape has been usually employed in this country, they were arranged as corrosives, irritants, astringents, narcotics, narcotico-acrids, and septics. Orfila rejected the first and third of these, Christison the group of septics also, while Taylor combines the narcotics and narcotico-acrids into a single division—termed neurotics—which, however, is subdivided into three subordinate ones, according as the substances contained in them act on the brain, the spinal cord, or both. It is a somewhat difficult matter to say what, under existing circumstances, would be the best method of classifying poisons; at all events we think ourselves justified in saying, that the natural-historical method adopted by Van Hasselt is a decided retrogression. The chemical plan has been employed by some, but although this suits very well in the inorganic division, it is altogether inapplicable to the vegetable and still more to the animal poisons, whose composition is in so many instances unknown to us. Another method, presenting some advantages in a country where there are limitations as to the sale of poisons, is by arranging them according to their virulence, but to us this would be useless. The mixed method, partly physical, partly chemical, of Devergie and others, may be also considered valueless in a scientific point of view. Such being the case, only two modes are left for selection—the *physiological*, depending on the part affected by the poison, and the *symptomatic*, which involves the symptoms educed by these actions. The former of these is recommended by Falck, who, however, only recognises two great divisions, nerve and blood poisons, while the latter is that adopted by Orfila, Christison, &c. The best arrangement would be one founded on these two, not like that of Dr. Taylor, who in one division includes all those poisons producing symptoms of *irritation* (irritants), while in another he places those acting more particularly on the *nervous system* (neurotics), but a truly pathological one. The time has not yet come for an exact and scientific classification of *all* poisons; our knowledge is far too imperfect to admit of such a thing; still, we gladly welcome an attempt at arranging them in such a manner as would give the practitioner who has to deal with a case of poisoning some clue to the substance which has produced the disturbance, as well as to the treatment most suitable for it.

The remaining chapters of his introduction Dr. Taylor devotes to the evidence of poisoning—first in the living, then in the dead. In the former case, the practitioner has a double duty to perform, that towards his patient and that towards the state; of the two, the former is the more imperative as long as life lasts, and it is of the greatest possible importance that the medical attendant should arrive at his conclusions as to the nature of the disorder as quickly as possible. As the work we are now discussing is limited to the subject of medical jurisprudence, and poisons are only looked at in their legal relations, we could scarcely expect the diagnosis of poisoning to be treated from any other than a legal point of view. In his work on Poisons, however, we think Dr. Taylor would have done well to point out the importance of an exact acquaintance with the conditions under which the patient lived previous to the attack, as such knowledge, in many cases, affords a clue to what would be otherwise inexplicable; he does, in one instance, refer to it (with reference to arsenic), but does not insist on it as much as we think it deserves.

Another point, scarcely adverted to by Dr. Taylor, is the importance of observing symptoms indicative of the action of some particular poison, Dr. Christison being of opinion that certain poisons can be made out by their symptoms alone, and instances strychnine, oxalic acid, &c., as examples. Upon the whole, we are inclined to agree with the latter; at all events the importance of a correct diagnosis is so great that a very close attention to the symptoms is demanded of the practitioner.

Authors differ considerably in the value they assign to the appearances found in the body of one supposed to have died of poison. As a proof of poisoning generally, they may be sometimes of no great value, but when viewed with reference to particular poisons they assume greater importance. Thus, in death arising from the exhibition of the stronger acids or the caustic alkalies, the post-mortem appearances usually occupy the first place, chemical analysis going to determine the particular substance employed, rather than that death had resulted from poison ing. One very important application of the morbid conditions observed in a case of suspected poisoning lies in their negative value, *i.e.* their power of completely refuting the charge in certain cases. This is hinted at rather than laid down by Dr. Taylor, though Casper justly insists upon its value. Thus, instances occur again and again where, from suspicious circumstances connected with the case, an idea of foul play has entered the public mind, and where an inspection of the body, by showing an entire absence of all traces of poisoning, along with the existence of morbid states sufficient to account for the train of symptoms, may readily settle the question altogether. The absence of abnormal conditions does not, however, negative the supposition of poisoning, except in cases where concentrated sulphuric acid, &c., are supposed to have been employed, for we occasionally meet with instances of arsenic having been given in considerable quantity without producing any marked change on the stomach or bowels, whilst the whole group of narcotics have hardly any action on the part to which they are applied.

Dr. Taylor gives some valuable rules for the guidance of the practitioner, as to what is to be observed, and what is to be done, in ordinary cases of death by poisoning; but we think he has made an omission of some importance, in not directing a portion of the blood to be preserved for analysis, nor do we believe that a partial chemical examination of the contents of the stomach at the time of the inspection (recommended by him) will be followed by any good result, but rather occasion a loss of material, which may give rise to future inconvenience.

The preceding are the only subjects relating to general toxicology discussed in Dr. Taylor's introduction, but there are several others not less important, although not alluded to in any English work on medical jurisprudence. Besides the symptoms during life, and the post-mortem appearances, the proof of poisoning depends in most cases on a chemical investigation and on the evidence of collateral circumstances. To the latter of these, belonging as it does rather to the lawyer than the physician, we shall not further allude, but the former presents so many points of interest as to deserve more than cursory remark.

One of the many vexed questions relating to forensic medicine is—"Who is the proper person to conduct the chemical investigation in a case of suspected poisoning—is it to be left in the hands of the medical man who made the post-mortem, or should it be entrusted to a purely chemical expert?" To either of these propositions forcible objections may be brought forward. We should think any medical man engaged in the ordinary practice of his profession very blameworthy should he undertake the chemical investigation even in an ordinary case, but more especially when a fellow-creature's life depends on the results. Two sets of mishaps are liable to occur: he is very likely to miss a poison really there, or to discover traces of substances which are not present in the body. The former of these would arise from an insufficient knowledge of chemistry, as pointed out by Schneider, and illustrated by the case of one attempting to destroy organic matter, containing common salt along with arsenic, by heating with sulphuric acid, when the poison would pass off as chloride of arsenic. On the other hand, by employing vessels which have been used before and not properly cleaned out, or by making use of impure reagents, the results may be falsified by the discovery of arsenic where no poisoning has taken place. Thus, commercial oil of vitriol usually contains a large proportion of arsenic, as does hydrochloric acid, even that which in some cases is sold as pure. Again, should the analysis be entrusted to a pure chemist, more especially to one who has had little or no experience in the detection of poisons, though the danger of such gross mistakes as those already alluded to be thereby avoided, questions are very likely to arise which cannot readily be answered by one ignorant of animal physiology, and he is also deprived of the valuable clue which the vital phenomena would afford to one skilled in their interpretation.

The most perfect association would no doubt be a physician well skilled in chemistry, with a chemist well acquainted with physiology; unfortunately this is not always attainable, nor are the English law authorities much inclined to afford those engaged in the chemical investigation that information which they possess as to the post-mortem appearances, &c. The absurd mystery with which they too frequently seek to surround the whole affair, so far as the chemists is concerned, apparently for the purpose of keeping him unbiassed, may operate disadvantageously, by concealing from him an important clue to the noxious substance. We cannot think that any man would be more likely to conclude that arsenic was present in any substance merely because the symptoms and morbid conditions indicated the action of an irritant, while such knowledge would doubtless lead to a more exact investigation in that particular direction, as well as to an important saving of materials. Another moot point on the Continent is, the desirability of having the superintendence of a law officer during the analysis; but as a proceeding of this kind is never dreamed of in this country, we need only hint at it.

Notwithstanding what has been said on its behalf, medico-legal chemistry is very far from being the accurate science it has been occasionally represented. There are limits beyond which the mere chemist cannot proceed, and where his researches leave the cause of death unexplained. Many substances which act as poisons are contained in the animal tissues as normal constituents; such are phosphorus, chlorine, &c.; and although the condition of these in the body is in a state of combination, still similar combinations are produced after poisoning has taken place, so that the substances give like reactions in both instances. Nor is this the only fallacy to which the chemical testimony is exposed, for the discovery of small quantities of such substances as copper or arsenic, prussic acid or oxalic acid, which do not normally exist in the human body, is not enough to prove that death has been caused by them; they may have been introduced into the system in food or medicine, intentionally or unintentionally. The only means of obviating such objections is by ascertaining, as exactly as possible, the quantity of poison contained in the body, and contrasting this with that contained in the substances in which it is said to have been administered. Caution is still more necessary in giving an opinion as to the cause of death in a body that has long laid in the ground, and has been examined in a state of decomposition, from merely chemical evidence, especially when only small quantities of the noxious substance have been discovered. The nails with which the coffin has been fastened, money where the body has been buried without a coffin, or it might be even the pins fastening the dead-clothes, would be enough to upset such evidence.

In his work on Poisons, Dr. Taylor makes a remark very well worth bearing in mind, viz. that the question is often put to the expert, whether enough poison has been found in the stomach to account for the person's death. Here it must be borne in mind, that in most cases (with the exception of the stronger acids, &c.) the poison found in the stomach is *that which remains over and above* the part which has been absorbed (and it may be eliminated), *i. e.* which has caused death, and is in fact that which has been left behind after vomiting and purging, provided any such symptoms have occurred. To estimate the total quantity found in the body would require a quantitative analysis of every portion of it, and even then we could not ascertain the amount that has passed out of the body, by the various secretions and excretions, after producing its peculiar effects.

Such being the position of medico-legal chemistry, it is fortunate that justice does not demand in every case the demonstration of the toxical agent; of this, prominent examples are afforded by the cases of Castaing and Palmer, who were condemned notwithstanding the deficiency, or rather total want, of chemical evidence. Nor is it only in the instances cited above, that we would object to the oft-quoted dogma of Plenk —"*Unicum signum certum dati veneni est notitia botanica inventi veneni vegetalis et analysis chemica inventi veneni mineralis,*" for were this the case, the great majority of vegetable poisons, as ordinarily administered (*i. e.* in infusion, decoction, or tincture), would entirely escape detection. Fortunately, for a considerable number of the vegetable poisons which have been properly investigated, what are called "*physiological reactions*" have been made out; for by repeated and careful observation it has been ascertained that the administration of certain substances to the lower animals is invariably followed by a peculiar train of symptoms and cadaveric phenomena. This knowledge has been applied to the detection of crime, as in the recent cases of De la Pommerais and Pritchard. In such cases this information, usually possessed by the physician and wanting in the pure chemist, is especially necessary. Nor is it enough that a certain quantity of material removed directly from the body of the supposed victim should have produced a fatal result in a dog or a rabbit; to prove that death has been caused by any substance it must first be purified, and the product thus obtained should give rise to symptoms and appearances similar to what the suspected substance would have produced in the same animal. Great care and caution are requisite in such experiments, or their results may be as negative as those afforded by chemistry. We are inclined to believe that to such researches we must chiefly look for our future advances towards a complete knowledge of animal and vegetable poisons, for even in cases where some of the alkaloïds which give characteristic reactions have been employed, the quantity necessary to destroy life, more especially when diffused through the whole system, is too small to be detected by chemical means. Another thing which should be borne in mind is, that the experiments demanded differ in their nature from those undertaken for purely physiological purposes; these are usually intended to elucidate some one particular, say with regard to the nervous system; those should aim at a knowledge of the morbid phenomena in their totality, so as when read backwards to give a clue to their cause.

A third means of investigation is offered by the microscope, and here a trained hand and eye are, if possible, more necessary than in the other cases; even as regards inorganic substances it affords invaluable information, as to their crystalline forms, their effects on polarized light, &c., but when we attempt to ascertain the nature of vegetable fragments found, it may be, in the stomach, its aid becomes indispensable. Helwig has of late years been attempting to bring it into more universal use as a means of detecting poisons, and has published a valuable series of photographs, exhibiting the characteristic shapes of a number of crystalline bodies used as such. The substances more particularly examined by him were arsenic, mercury, antimony, lead, zinc, silver, the alkaloïds, &c., and in this way he believes himself capable of distinguishing $\frac{1}{1000}$ gr. of arsenious acid. Another mode of detection which he hints at, rather than enlarges upon, is the chemical reactions of very small quantities of a substance as viewed through the microscope; this we think a rather promising field of research, more especially as regards those bugbears of the toxicologist, the alkaloïds.

It is greatly to be regretted that in no English work on Toxicology do we find a scheme laid down for a systematic analysis in a case of poisoning. In connection with each substance the tests to be employed, and in some cases the analytical process applicable to it, are described, but it is evident that these are only available when the poison itself is known, which is but seldom the case. Under such circumstances we should have been glad to find some directions supplied by a toxicologist of Dr. Taylor's standing and experience, and cannot help thinking it unfortunate that he has not given them. Works on practical chemistry usually contain schemes for the systematic conduct of a chemical investigation, but in general they limit themselves to inorganic substances, and rarely include the proximate analysis of vegetable compounds. There are two German works specially devoted to medico-legal chemistry, one by Schneider, the other by Otto; the former, more especially, containing an immense amount of practical information, which, as we have again and again ascertained, may be thoroughly relied upon. Unfortunately, from the date of its publication (1852), it cannot contain the results of the latest investigations. Otto's work, considerably smaller than that of Schneider, is more strictly limited to toxicological chemistry, out is more available to English readers, having been translated by Dr. Elderhorst, of Troy, U.S., who has added some valuable matter to it. Several of the German works on Toxicology, as the translations of Van Hasselt, &c., also include directions for a systematic analysis, but in most instances they do not sufficiently contemplate the necessity of saving the material to be experimented upon. Otto, in an Appendix, gives an outline process which has some regard to this, but it is scarcely comprehensive enough to include all the substances it may be necessary to seek. When the substance to be examined is not mixed up with organic matter, the process is tolerably simple; but when the contents of the stomach, the liver, or blood, are the subjects of investigation, the separation of the poison in a state of purity is attended with considerable difficulty.

The following process will, we think, be found to include the means of detecting most of the known poisons, and possesses the advantage of sacrificing none of the material operated upon. It will usually be found that researches after the poisonous gases prove abortive, unless such substances as arseniuretted hydrogen have been absorbed, when arsenic may be discovered in the blood by the ordinary process; they may, therefore, meanwhile be left out of the scheme. When the contents of the stomach have to be examined it is necessary, in most cases, to dilute with distilled water until they attain the consistence of a thin pulp; their acidity or alkalinity may be ascertained in the ordinary way, and the whole placed in a tubulated retort, or an alembic provided with a well-fitting capital. A gentle heat should be employed in the first instance, never exceeding that attainable by an ordinary water bath. By this process such volatile substances as chloroform, ether, alcohol, free prussic acid, &c., may be collected in a receiver containing some water either pure or mixed with caustic potass, which should after a time be changed, the contents of the retort rendered distinctly acid by glacial acetic acid, and some lumps of sulphur added. The size of each of these pieces should not exceed that of a hazel nut, that of a horse-bean being the most convenient. A fresh receiver, also containing some caustic potash, should now be attached to the retort, and a moderate heat applied, as before. Should any metallic cyanide be present, the acid will pass over, and become absorbed by the caustic potash in the receiver, which will then yield characteristic results on being treated by the proper reagents. Of these, the Prussian blue test is the most certain; Liebig's test also yields characteristic results, but the reaction with nitrate of silver is apt to be obscured by the excessive alkalinity of the fluid except the vapours passing out of the retort be employed, and might in ordinary cases be confounded with that of hydrochloric acid.

By this plan we have succeeded in obtaining characteristic reaction when the retort contained but a very minute quantity of cyanide of potassium. When the presence or absence of prussic acid has been determined, the retort should be lowered so that its bottom dips into the water in the bath, which should now be made to boil, and thus determine the union of the phosphorus, should there be any present, with the sulphur previously introduced. Part of the phosphorus may also pass over into the receiver in the shape of phosphorous acid. To effect the union of sulphur with phosphorus, a temperature under 212° F. suffices, and, in fact, anything higher would prove dangerous, as the phosphorus is then apt to explode. After removing the pieces of sulphur, they should be well washed, and then heated with strong nitric acid; this will oxidize the phosphorus should there be any, converting it into phosphoric acid, which may be recognised by the usual means. These preliminary processes over, the vegetable alkaloïds may now be sought, for which a modification of the process recommended by Stas will be found the most suitable. Instead of using tartaric or oxalic acid, as recommended by that chemist, the acetic acid already employed will probably be found more serviceable, yielding, as it does, a salt more soluble in water than those formed by the previously mentioned acids, and possessing the additional advantage of not introducing a poisonous element, whose existence has not yet been determined in the substances under examination. In this process there are frequently certain obstacles to be overcome, depending chiefly on the difficulty of separating any fatty matter derived from the tissues, and, when blood is employed, of preventing the corpuscles from passing through the filter. Both, however, may be overcome by using a little extra care. After adding a little more acetic acid at this stage, the whole may be heated over the water bath till the liquid portion has completely evaporated; the blood-globules present will thus be broken up, so that on the addition of some water acidulated with acetic acid, and after throwing the whole upon a filter, if any colouring matter pass through, it will consist of hæmatine alone, which can be readily disposed of at a more advanced stage of the process. The substances remaining on the filter should be washed with hot alcohol acidulated with acetic acid, and the fluid part added to that which has already passed through. The whole fluid collected should be permitted to stand until it has cooled, when if any fat be present it will form a cake on the surface, and may thus be easily removed. The filtered liquid should then be evaporated to dryness, again rubbed up with water or alcohol, and again filtered; on a third evaporation it will usually be found tolerably pure, but should it not be sufficiently so, another filtration and evaporation may be had recourse to. The liquid should now be neutralized, or even rendered slightly alkaline by the bicarbonate of potash or pure carbonate of lime, the free acid expelled by heat, the whole rubbed up with chloroform, placed in a tall narrow test-tube, and allowed to stand, well corked, for some hours, after which the chloroform containing the alkaloïd may be removed from beneath the watery layer of fluid, and allowed to evaporate spontaneously. As a solvent for the alkaloïds chloroform is in most cases greatly superior to ether or benzole, but from its being found below the watery portion of the fluid is more difficult to remove uncontaminated. As a permanent solvent of most alkaloïds a mixture of alcohol, ether, and chloroform, is extremely useful; but where separation from water and speedy evaporation are required, such a mixture will be found useless. Should benzole be employed, care must be taken not to evaporate it over a flame, or its vapour may take fire and ruin the process. The residue, after separating the pure alkaloïd, may be tested for such substances as oxalic acid, &c.

Much has of late years been said and written on the subject of the poisonous alkaloïds, and although their chemical reactions are scarcely well enough known as yet, still a gleam of daylight begins to be shed upon them. Even supposing that one of them does exist in the chloroformic solution just referred to, except some clue be afforded by the symptoms, &c., or a regular analytical process instituted, the materials may be wasted before any definite result is attained. To this end, that is, to distinguish the various alkaloïds, various analytical schemes have been proposed, one by a M. Valser, contained in Briand and Chaudé's excellent 'Médecine Légale,' the latest and best by Dr. F. C. Schneider, the accomplished author of the "Gerichtliche Chemie," in the 'Wiener Mediziner Wochenschrift.' The former of these, we are sorry to say, is altogether useless, but the latter corresponds in almost every respect with our own experiments executed some time ago. The two great classifying tests are sulphuric and nitric acid, cold and hot, which suffice for the separation of the greater number of these substances, though some subordinate reactions will be found useful for the discrimination of one or two of them. Aconitia and atropia are two alkaloïds not easily distinguished from each other, but the plan just mentioned affords a tolerably ready means of doing so. Atropia when heated with concentrated sulphuric acid evolves an odour similar to that of wild honey, or of preserved rose leaves, while aconitia gives rise to suffocating fumes. Daturia gives exactly the same reactions as atropia. We are not aware that these peculiarities have hitherto been made public, but from frequently repeated experiments are convinced of their accuracy. The employment of sulphuric acid for such a purpose shows the great impropriety of using this substance, as is sometimes done, for destroying any organic matter which may be mixed with the alkaloïd. Strong oil of vitriol, such as is made use of with this view, always partially decomposes the alkaloïd, and if bichromate of potash be added to such a solution, especially while hot, it will infallibly give a fallacious result.

The substances left behind on the various filters are next to be collected, and examined for the ordinary mineral poisons. The process recommended by Fresenius and Von Babo for the destruction of the organic matter is that most frequently adopted, as it is certainly the most suitable. Hydrochloric acid and chlorate of potass with heat are used for this purpose, the metallic oxide, if there be any there, is then reduced from its highest degree of oxidation to a lower by sulphite of soda, and finally thrown down (if possible) by sulphuretted hydrogen. The subsequent steps are the same as those employed in ordinary systematic analysis, and need not be further described here. The only precaution of very great importance here necessary, so far as the initiatory steps are concerned at least, is the ascertaining the purity of the hydrochloric acid, which is very often contaminated with chloride of arsenic, derived from the sulphuric acid employed in its manufacture. Such is an outline of the process we would propose as most suitable for the systematic analysis of any suspected matter in a case of poisoning. But we must turn to another department of our subject.

In his present work, Dr. Taylor is also silent as to the way in which poisons act upon the system, though in his 'Toxicology' he discusses the subject at considerable length, and has there collected a great number of valuable facts as to the mode and period of their elimination. It would be difficult to over-estimate the importance of such data, in cases where the possibility of finding a certain poison in the system after a given time is affirmed or denied in a court of law. There are, however, certain subjects omitted which we should have been glad to see discussed, not only from their scientific value, but also from their affording indications as to the mode of treatment.

The oft-vexed question as to how poisons act on the body at large, whether through the vascular or nervous systems, seemed to have been at last settled in favour of the former, as it was supposed to have been proved that they could not act till they had been absorbed, and so brought in contact with the nervous centres. Not to cite the classical names of Majendie, Brodie, &c., in favour of this doctrine, it has in more recent times been strongly advocated by Bernard, Harley, and Kölliker; but if we are to put any reliance on the experiments of Martin-Magron, experiments apparently performed with the greatest possible care, and to which we shall have occasion to refer hereafter, we must accept the doctrine that strychnine can act not only directly on the nervous centres, but that too after they have been entirely deprived of blood by careful washing. Notwithstanding this, it may be accepted as sufficiently established that the usual way for poisons to act is by absorption, and that the rapidity of their action is directly proportionate to this, varying, however, with the nature of the poison and the mode of its application. Most toxical substances have a twofold action, one on the organ to which they are applied, and another on the system at large, the one direct, the other indirect; the former of these varies excessively sometimes, as in the case of the mineral acids, being by far the most prominent feature; in others, as in poisoning with opium, prussic acid, &c., being either entirely absent, or so masked as to be almost inappreciable. The secondary or general action will of course vary also, but we hardly think that sufficient attention has been paid to the importance of distinguishing between its *two very distinct varieties*. The one is that constitutional reaction which occurs whenever the organism receives a shock in any of its members, sometimes known by one name, sometimes by another, in surgery generally distinguished as surgical fever. Or it may be that which precedes the reaction and gives rise to it, usually known as *shock*. Of these we have well-marked illustrations in the oft-cited action of the *corrosives*, which may either cause death immediately by the latter, or lead to it through the former, that is to say, inflammation and its consequences. The action of sulphuric acid is the same whether applied to the exterior or the interior of the body—no doubt exaggerated in the latter case, but still the same in kind—and no distinction can be drawn between its constitutional effects in the one instance and in the other. The second variety of remote or secondary action differs from the preceding essentially in depending on some specific effect of the poison, altogether apart from what may be considered the result of its local action; thus, dilatation of the pupil by belladonna and hyoscyamus, and its contraction by opium, however exhibited, have nothing to do with the sedative action which these substances exercise on the part to which they are applied. Intimately connected with this last variety is the affinity which certain substances have for particular organs, whether they are exhibited externally or internally. Not that the same poison invariably attacks the same part of the body, for the organ affected differs in a marked degree with the mode of its administration, whether introduced into the alimentary tract, or directly into the system, as in snake bites, and especially according to the kind of *intoxication*, (as continental writers are fond of calling the state produced by any toxical agent,) whether it be acute or chronic—well seen in the case of lead. A few examples of this peculiar affection, shown by certain poisons for particular organs, may be interesting to those who have not made the subject a special study.

Some substances, though not very many, attack the bones more than any other part; thus mercury, more especially when combined with the syphilitic cachexia, frequently produces caries, or gives rise to periostitis, and phosphorus, when taken into the system for any length of time, produces caries of the lower jaw. Occasionally, though more rarely, the joints are affected in a somewhat similar manner, but the poisons which influence them are most frequently produced in the body itself, as in gout and rheumatism. The muscles are also liable to be affected either by supra-excitability, or by paralysis more or less complete; their nutrition may also be interfered with, giving rise to atrophy or degeneration. The affections of the muscles themselves are, however, to be distinguished from those of the nerves which supply them, although the symptoms are somewhat similar. Thus, tartar emetic, by acting on the nervous system, gives rise to weakness of the limbs, and a general feeling of lassitude; white lead again produces atrophy with paralysis, or convulsive contractions of the muscles of the forearm; chronic alcoholism, muscular atrophy or fatty degeneration. On the central organs of the nervous system many substances exercise a well-marked influence, others again act upon the nerves or peripherical organs; some, like picrotoxin, produce convulsive movements; digitalin stimulates the vagus, and lowers the action of the heart; aconitin attacks the sensory nerves, and gives rise to anæsthesia; veratria and delphinia cause formication and tingling; while the various effects produced by the different narcotic substances are far too numerous to be mentioned. The lungs are implicated by a great number of poisons, some being eliminated by them, others attacking their tissues; thus alcohol, ether, chloroform, prussic acid, conia, nicotina, camphor, ammonia, and the compounds of allyl escape by them, while tartar emetic produces inflammation of their substance. Some attack the heart, as digitalin (directly or indirectly), delphinia, nicotin, &c., producing paralysis more or less complete; camphor, strong coffee, arsenical and ammoniacal salts, as well as many ethereal and empyreumatic oils, weaken its power of contraction; mercurial, arsenical, spirituous and animal poisons affect the nutrition of the same organ, and give rise to inflammation, degeneration, &c. The lungs are affected in a similar manner, by the same substances, whilst snake poison gives rise to paralysis of them, and thus leads to death. The alimentary canal is liable to various kinds of derangement from similar agencies. The mouth is attacked by mercury, iodine, and lead; the stomach and bowels by the whole series of the irritants, and in an especial manner by arsenic, antimony, and the other emetics, which produce their peculiar action, even although they have been introduced directly into the veins. Cramp and paralysis of the bowels follow the introduction of lead into the system; increase of their peristaltic action is produced by delphinia, gamboge, &c; diminution of the same by opium and lead; and impaired nutrition by putrid substances. The liver is acted upon by a great variety of substances, and many poisons may be detected in it when their traces are almost imperceptible in other organs. Manganese, zinc, copper, and arsenic, are notorious instances of this peculiar affinity. A reason for such accumulations has been sought in the liver, in the great vascularity of that organ, but it does not depend on this alone; to explain it properly, the peculiarity of its circulation must be borne in mind, viz., that the whole of the blood which has circulated over the alimentary canal, and which contains the poisonous matter absorbed thence, is conducted by the portal vein into the liver, and only escapes from it by passing through a multitude of capillaries, surrounded by cells in a high state of activity. The wonder is rather that any should pass through than that so much should be retained.

The kidneys, as the great emunctories of the body, will naturally be attacked by many poisons; but some, as cantharides, turpentine, and corrosive sublimate, influence them in an especial manner. The effects of chronic alcoholism on both liver and kidneys are too well known to require more than passing remark.

Another subject of much interest is the comparative actions of poisons. Most toxical substances differ in their effects upon the system, and may be termed *allodynamic;* others so closely resemble each other that their actions might be considered identical; such are called *isodynamic.* Some, while not exactly corresponding in their effects, are still so closely approximated as to justify us in naming them *homoiodynamic;* while a fourth group are characterised by their antagonism, and may be termed *antidynamics*, or counter-poisons.

The group of poisons having identical effects (*isodynamics*) is very limited, being chiefly composed of salts, which owe their toxical effects to the same base, or to the same radical, such as the sulphate and nitrate of copper, the sulphate and nitrate of silver, the nitrate, chloride, and acetate of baryta, the iodides of potassium and sodium, the alkaline oxalates, and the alkaline sulphides; but it also comprehends substances quite distinct in their chemical properties, such as caustic potash, and caustic soda, (although the action of these upon the body depends on the same chemical affinities,) as well as gallic and tannic acids,—quinine and cinchonine,—atropia, daturia, and hyoscyamia, and, to a certain extent,—strychnia and brucia. The *homoiodynamics* are more numerous, comprehending the mercurials,—the antimonials,—the arsenicals, as well as—nicotine and coniine,—delphinia and veratria—strychnine and picrotoxine, &c. The importance of recognising such a class as this depends on the effect they have of exacerbating each other's action should they be exhibited either simultaneously or after each other. *Allodynamic* poisons are so numerous as to require mention only; as examples, we might cite caustic potash and prussic acid—carbonate of lead and tartar emetic, and so on. The group of *antidynamics* is, in reality, a subdivision of this last, comprehending such substances as possess actions so very dissimilar as to be antagonistic, and is very limited in extent; we may cite as examples strychnia and conia,—and opium and belladonna.

The consideration of the last division leads us to a subject which has been neglected by most of our systematic writers on toxicology, and is discussed in neither of Dr. Taylor's works—*the general treatment of poisoning.* It is not enough for the practitioner to know what substances are best adapted as antidotes to particular poisons, for the time may come when these substances are not attainable, or when the nature of the poison itself is unknown. Under such circumstances, a knowledge of general rules is all-important, and, combined with ingenuity on the part of the medical attendant, may succeed in preserving a life which would otherwise have been sacrificed. The absolute rules ordinarily laid down for the practitioner's guidance are open, as usual, to several exceptions. The programme usually prescribed is, first of all, to remove the poison as soon as possible; to neutralize and prevent the absorption of what cannot be removed; to promote the elimination of what has been already absorbed; and finally, to treat symptoms as they occur. This, however, has been drawn up entirely with reference to poisons taken into the stomach, and requires to be considerably modified when applied to such as are introduced directly into the system. There has also been some little dispute as to whether *neutralization* or *removal* should be first attempted—in reality, the rule must be modified by circumstances; the old dictum, that "the readiest remedy is the best," overpowers every other consideration. If the means of removal be at hand, as they usually are, by all means get rid of the poison at once; but if one should have to wait for half an hour till a stomach-pump be obtained, let him attempt the neutralization of the poison immediately. There are three emetics, one or other of which is sure to be attainable, and either of which will produce an efficient action of the stomach. These are common salt, mustard, and smelling salts, exhibited, the first, in the dose of two or three table-spoonsful, the second of a single table-spoonful, and the last, of about a tea-spoonful, given in lukewarm water in each case. But, should the stomach, as is often the case in opiate poisoning, be too much oppressed by the sedative action of the drug to respond to any ordinary stimulus, the general treatment should be proceeded with, if a stomach-pump be not at hand. Our first steps should always have reference to the peculiar action of the poison, and to the mode in which it has been applied. When, for instance, its action is chiefly local, as in the case of the stronger acids, our chief care should be directed towards preventing or remedying this, which can be most readily effected by neutralizing agents, any attempts at removal, more especially by the stomach-pump, being altogether contraindicated. This instrument, which unfortunately is looked upon by some as a panacea for every poison, should never be made use of where the local action of the poison is excessive, as is the case with the whole group of what are termed the corrosives. It is not very long since we saw a case reported in one of our weekly periodicals, where it was used in poisoning with chloride of zinc; the very worst fate we could wish the authors of this ingenious mode of torture is, should they ever be unfortunate enough to swallow a poisonous dose of the same substance, that they may be treated according to their own prescription. When again the poison, though acting but slightly locally, is introduced directly into the system, as in snake bites, it is of greater consequence to prevent its absorption than to attempt its immediate removal. The venous current of the blood should be arrested by ligature or pressure, before any attempt is made by washing, sucking, cupping, or cauterizing to purify the wound from its dangerous contamination. So also, should the toxical agent have been introduced through the lungs, as in the case of such gaseous poisons as carbonic oxide, or arseniuretted hydrogen, we cannot effect their removal save by promoting free respiration in the open air; nor can we prevent the blood from becoming affected; our whole efforts should therefore be directed to strengthening the system sufficiently to enable it to throw off the injurious agent, or to warding off, as far as we can, the evil consequences likely to follow its intussusception. These illustrations are sufficient to show that in this as in other departments of medicine no single rule is absolute, or of paramount authority.

The mode of treatment most in vogue is that founded on the chemical properties of the poison, the object in view being either to convert it into an inactive or a harmless compound. The substances used for this purpose are termed *antidotes,* and may be arranged in three groups—the *chemical,* the *static* or *physical,* and the *dynamic* or *physiological.* The two former, it is obvious, can only be employed with any great hope of success before the poison has had time to be absorbed; the third, which scarcely merits the name of antidote at all, has reference only to the constitutional effects of the poison, and may therefore be employed at any time. In former ages one of the grand objects of research was a universal antidote, one capable of neutralizing the evil consequences of every poison. Bezoars were supposed to have an important action of this kind, and the mithridaticum, the last remnant of which has but lately been expelled from general practice, owed its origin and employment to a similar supposition. Nor has the search after such alexipharmics been confined to antiquity; we have instances, in the most recent times, of men bringing forward some favourite remedy as a panacea for all poisons; in the list we have such diverse substances as acetic acid, tannic acid, pectic acid, magnesia, soap and charcoal, as well as albumen, and chlorine, and it is but the other day that we had certain authorities extolling the mixed oxides and carbonate of iron as a neutralizer of three different poisons. Unfortunately, men forget that the stomach is not a test tube, and that they have to do with something else than pure water as a solvent, as they may find should they ever come across a case of poisoning by orpiment, Scheele's green, or even calomel. For this reason also must all the substances exhibited as antidotes be capable of acting at the temperature of the stomach; for instance, grape sugar would be useless as a remedy where a salt of copper has been employed, seeing that it requires a higher temperature than that of the body to reduce the copper to the condition of suboxide. Another rule to be attended to is, that no substance which is itself dangerous should be employed as an antidote to another noxious agent; there are, however, certain exceptions to this one which will be alluded to hereafter.

In reality, the number of perfect antidotes is much more limited than is ordinarily believed, and there are but few poisons which are capable of complete neutralization. The whole group of animal and vegetable poisons may be said to be without a single antidote, although there are certain substances which may be employed in the case of the latter with some slight hope of success. One of the best of these is tannic acid, and substances which contain it, as green or black tea, &c. This throws down almost every one of the alkaloïds, but some of these precipitates are pretty readily re-dissolved by the weaker acids. Another substance, which offers well as an antidote for the same class of poisons, is a solution of iodine in water containing iodide of potassium—the old Liquor potassii iodidi compositus; but this, though preferable as a neutralizer, is not so often at hand as tannin, or substances containing it.

Mechanical, or static antidotes, are such substances as having no special action on the poison itself, yet, by becoming mixed up with it, or by covering the walls of the stomach, serve to retard its action. The simplest of these are *diluents,* especially those of a mucilaginous nature, such as water, gruel, milk, and gluten, though the two last may in certain cases (the salts of mercury for instance) act chemically. The action of the mineral acids may be arrested by simple dilution, but it must be borne in mind that where the effects of the poison are chiefly secondary, diluents, as tending to promote absorption, will favour rather than delay the *intoxication.* This is notoriously the case with oxalic acid, which acts more energetically when diluted than when concentrated; in the former case, its action on the system at large is more marked than in the latter. When, however, the local action is but slightly marked, when the poison has been introduced directly into the circulation, or applied in such a way that it can neither be neutralized nor removed, or when its effects supervene too rapidly to admit of any such treatment, recourse must be had to *physiological or dynamical antidotes.* Examples of such substances are afforded by ammonia and chlorine, used in cases of poisoning by prussic acid; chloroform in poisoning by strychnine; as well as by the whole class of antidynamics already alluded to. One of the most curious illustrations of this mode of treatment is seen in the application of belladonna as a counter-poison to opium. The suggestion which originally led to its employment for this purpose was thrown out by Corrigan, followed up by Graves, Anderson, Garrod, and Bell, and was founded on their antagonistic effects on the pupil of the eye. Since Mr. Bell's paper appeared it has frequently been employed in this way, cases having been reported by Dr. Taylor and others; and seeing that it can be used where everything else has failed, even in the most unpromising cases, where, though the patient be unable to swallow, hypodermic injection may be used, it is well worth remembering. Founding an opinion merely upon their respective actions on the pupil, we should not be surprised should belladonna turn out an antidote to the Calabar bean, and would suggest that those who come across cases of poisoning by the bean should bear in mind the action of belladonna in the case of opium. Another illustration of a counter-poison was supposed to exist in the relative effects of strychnia and curara, but recent investigations have shown that the antagonistic action is apparent rather than real. In certain cases, as where opium has been taken, strong coffee is recommended; its action here is probably to some extent due to its stimulating powers, but more likely to the amount of tannin contained in it, so that it belongs rather to the chemical than to the dynamical class of remedies.

With these remarks we must at present content ourselves, but we shall take up the subject of Special Toxicology in a future number.

REVIEW II.

On the Cure of Club-foot without Cutting Tendons, and on certain New Methods of Treating other Deformities. By RICHARD BARWELL, F.R.C.S., Assistant-Surgeon, Charing Cross Hospital. *London:* Robert Hardwicke. 1865. Pp. 228.

THE title of this book is somewhat startling. We have been so accustomed of late years to regard the division of tendons as essential to the cure of the severer cases of club-foot—nay, more, we have been so much in the habit of looking upon the practice of subcutaneous tenotomy, which was introduced by Stromeyer in 1832, as one of the greatest improvements in modern surgery, that a book which undertakes to tell us how club-foot may be cured without cutting tendons can hardly fail to arrest our attention. We take it up with interest, and perhaps with some little incredulity. We are inclined to think that it may be nothing more than one of the many "new methods" of treatment which are constantly being proposed for our consideration, but so few of which stand the test of a more extended trial.

Mr. Barwell has, however, shown good cause why we should reconsider the practice of tenotomy. He has, we think, proved beyond all question that it is at present used to an extent which is unreasonable in theory and unsatisfactory in its results. After he has arrived at this conclusion, it is clearly his next duty to suggest a better plan of treatment. When our guide tells us that we stand upon unstable ground we expect him to show us the *terra firma*. Accordingly, our author goes on to explain a method of treatment which he has originated, and which appears to be both rational and physiological. If we may judge by the few cases which are detailed in the volume before us, the "new method" seems to be easy of application, free from pain or danger, and satisfactory in its results.

Any method of practice which professes to accomplish without an operation that which has hitherto required the use of the knife deserves our most attentive consideration. Even in these days of anæsthetics patients shrink from being cut. We have all an instinctive dread of an operation, however slight it may be; and the public are well aware that no wound is altogether free from danger. Under these circumstances it is our duty to try and reduce the number of operations as far as possible, and to find safer and more painless methods of carrying out our objects.

Half a century has hardly elapsed since the idea of perforating or crushing stones in the bladder was mooted, and it was only in 1824 that Civiale performed his first operation of lithotrity. Before that date every patient afflicted with stone had been subjected to the knife; but now, in every case of the kind which we meet with, the question arises whether we shall have recourse to lithotomy or lithotrity. What a great improvement is this! A method has here been introduced which enables us to deal with a large number of cases of stone in the bladder without the use of the knife, and without exposing the patient to the pain and the danger which are inseparable from the cutting operations.

In 1785 Hunter first put a ligature upon the superficial femoral artery for the cure of popliteal aneurysm—a disease which before that time had almost always necessitated an amputation. This was a great step in advance. It substituted a comparatively small operation for a much larger one, and it left an entire limb in place of a mere stump. But we have not rested here. Other steps have been taken in the same direction, and Hunter's simple and beautiful operation has now been superseded by compression—a method which is not only simpler, but which has the great advantage of dispensing with the use of the knife altogether.

No better proof could be given of the progress which surgery has made during the past century than that the two formidable complaints we have mentioned—stone and aneurysm—may now sometimes be cured with little or no pain, and without the loss of a single drop of blood.

If, therefore, Mr. Barwell can succeed in showing that the severer cases of club-foot can be cured without cutting the tendons, he will have conferred a great boon upon surgery. It is true that the operations which have hitherto been practised for the cure of club-foot are comparatively trifling ones. The subcutaneous division of one or two tendons is a very small matter, and one which is attended with the least possible amount of danger. If this practice were quite satisfactory, we might be well contented to abide by it. But is it quite satisfactory? Our author replies in the negative. The operation is somewhat uncertain in its effect—the after-treatment is tedious, and requires cumbrous and costly appliances—while it would appear that there is good reason to question the ultimate results.

At page 42 Mr. Barwell gives an analysis of nine cases of tenotomised patients, who have subsequently died from other causes, and whose feet have been examined *post-mortem* with special reference to the state of the tendons divided at the operation. These cases have been collected by Mr. W. Adams, and published in his work 'On the Reparative Process in Human Tendons.' From the analysis it would appear that, with the exception of the *tendo-Achillis*, the chances are either that the divided tendon will not unite at all, or that it will become attached to some of the surrounding structures. If either of these accidents should occur, the use of the tendon is of course destroyed, and the corresponding muscle is virtually paralysed.

Again, at page 45, our author draws special attention to a case in which the surgeon determined to divide the tendons of the tibialis posticus, and then apply a mechanical shoe. The operation was performed, the shoe was applied, and the treatment seemed to succeed. But after the patient's death it was found that the tendon had never been divided at all! We all know that some slight cases of club-foot may be reduced to position without cutting the tendons; but here was a case in which subcutaneous section was thought necessary, and yet after the treatment had succeeded so far as to satisfy the surgeon, it turned out that the tendon had escaped the knife altogether!

Such facts as these—the uncertain issue of an operation, and the possibility of reducing a severe case of club-foot without dividing the contracted tendon—may well incline us to review the present practice of the tenotomists, and to listen with an attentive ear to any other plan of treatment which is proposed for our consideration.

If we look at an ordinary case of *talipes valgus*, or splay-foot, we can hardly fail to be struck with the appearance of weakness and relaxation which the whole ankle and foot present. There is evidently a want of proper force and tone in the tissues.

Such being the case, it certainly seems unreasonable to divide the contracted tendons—which is in effect to paralyse those muscles which still retain their contractile power—and yet the free division of the tendons, and the application of a "Scarpa's Shoe," is the treatment which is commonly recommended in these cases. Now, it is the soundness of such practice as this that Mr. Barwell calls in question.

It will be observed that we are speaking of those cases of club-foot which depend upon paralysis, and we apprehend that they form the largest class with which the orthopœdist has to deal. Assuming, then, that the common cause of the deformities which we are considering is paralysis—that the muscles have lost something of their contractile power—the free division of tendons, and the application of a firm, unyielding shoe, does not seem likely to restore the muscular force or to strengthen the ankle-joint. Moreover, the foot is a complex organ, composed of several distinct portions, which are articulated together; and it is undesirable to act upon it as a whole, in the way that the mechanical appliances so often do; but it should be treated as consisting of at least two separate parts. Following out this train of reasoning, Mr. Barwell has asked whether tenotomy and the application of a mechanical shoe is necessary at all, even in the severer cases of club-foot. Does not such treatment add to the mischief? Does it not intensify the disease, which is essentially one of debility? This question the author answers in the affirmative; and then he proceeds to explain the method of treatment which he has devised, and which consists in supplying the place of the paralysed muscles by means of elastic cords. By a light and simple contrivance these cords are fastened to points as near the origin and insertion of the paralysed muscles as possible, and the patient is encouraged to use his limb, while at the same time constitutional remedies are prescribed, and local means, such as friction, galvanism, &c., are employed. Such is, in brief, the new method of cure for club-foot which Mr. Barwell has proposed, and which he has practised with apparent success during the last few years. Certainly it commends itself to us as a rational and physiological plan of treatment. It proceeds upon the principle, that our aim ought to be to strengthen weak parts, and not to weaken strong ones—a principle which is gaining ground every day in the practice of medicine and surgery.

But is this method of treatment applicable to all cases of club-foot? We can hardly suppose that it is. Even if the principle is found to hold good to the extent which its author anticipates, there will always, we imagine, remain a balance of cases which will require tenotomy and the use of a mechanical shoe. Congenital malformations, contractions following injuries, abscesses, &c.,—these cases will never be amenable to Mr. Barwell's plan; although after division of the tendons the use of elastic cords has been found of great service. We should have been glad if our author had discriminated more carefully the cases to which he considers his method is most applicable. This is a point of great importance in recommending a new plan of treatment. Nothing can be more damaging to an experiment than that it should be tried upon an unsuitable subject. We would therefore advise Mr. Barwell to lay down clearly and distinctly the cases which he finds can be cured by his method, and to mention the classes of cases to which it is less applicable. At the present day, when the resources of our art are so numerous, the highest surgical skill consists in a wise discrimination of cases, and in the selection of the means of cure most suitable to the particular case in hand. Every one must have noticed how prone men are to fall into a routine of practice, and to deal with groups of disease rather than with individual cases.

In some of the severer cases with which he has dealt, Mr. Barwell has found it a good plan to begin the treatment by putting the patient under chloroform, and making rapid extension. By this means he has been enabled, after the lapse of a few days, to apply the elastic cords with greater advantage, and to expedite the cure. But we should be glad to learn whether our author finds himself able to deal with the severest class of paralytic cases by means of his method. Given an aggravated and long-standing case of club-foot from paralysis: are the elastic cords sufficient to effect a cure? None of the cases that are detailed in the volume before us give a sufficient answer to this question; for they are either cases of children, or cases in which the disease had not long existed.

Following up his subject, Mr. Barwell has adapted his method to the treatment of knock-knees and crooked shins; and he has devised means by which spring force may be brought to bear upon these deformities in such a way as to draw the bones into a more natural position. An ingenious instrument for the cure of knock-knees is described and figured at page 210. It does not, however, appear to us to be so happily devised as our author's method of acting upon club-feet. It stands out so far from the leg that one would think a boy could hardly wear it at all; moreover, it is rather complicated, and must interfere with progression. Might not the same principle which has been applied to club-foot, namely, the principle of strengthening the weak side, be employed in treating knock-knees? Might not a broad india-rubber band be stretched down the inside of the leg, while the outside was, if necessary, supported by a light splint? Would not such a plan be more in harmony with the physiology and pathology of the case than the method here described?

When we were speaking of the unsatisfactory results which often seem to follow the division of tendons, we made an exception in favour of the *tendo-Achillis*. This tendon, by reason of its size and its isolated position, unites readily and with comparative certainty. There cannot, therefore, be the same objection to cutting it that there is to cutting the other tendons; and we believe it is in simple cases of *talipes equinus*, that the best results of tenotomy are seen. Speaking of this operation, Mr. Liston says, in his 'Practical Surgery,'—

> "Twenty-five instances of division of the tendon have come under our care within the last six or eight months, and an almost uniform success has followed the practice pursued. In some a rapid cure was effected, and in all marked good effects followed. Even in the most unfavourable instances, when the deformity was both great and of long-standing, the application of proper apparatus within a week or two after the operation, produced a more decided improvement in the form of the limb in two or three months than usually follows the adaptation of mechanical means alone in twice as many years."

It would appear, therefore, from this testimony among others, that the subcutaneous division of the *tendo-Achillis* is quite satisfactory. It is the practice of dividing the secondary tendons about the ankle-joint which is open to a question. It is not at all improbable that tenotomy may have been carried too far in this direction; and if the publication of Mr. Barwell's book leads to a revision of the whole subject, the cause of scientific surgery will undoubtedly be advanced.

The volume before us is illustrated with a number of very indifferent photographs. There are besides a few woodcuts, which are good of their kind; had they been rather more numerous we could well have dispensed with the photographs altogether.

Mr. Barwell's style is not always so clear and lucid as we could have desired, and his sentences are often very deficient in literary finish. We have also to complain of the use of such words as *contractured*, *debile*, *deformation*, and *fautor*. They are pedantic and out of place in a book like this.

But the most serious fault that we have to find with Mr. Barwell's writing is, the way in which he speaks of the tenotomists and their methods of treatment. It is irritating, and calculated to give unnecessary offence. Our author has originated a sensible and satisfactory plan of treatment, and it is now his aim to introduce it to the profession, and to get it tried on a more extended scale. Such being the case, let us assure him that he will carry out his object far more certainly by temperance and moderation than by using a vehemence which only serves to arouse the spirit of opposition in those who differ from him. By calmly and courteously advocating his views he is sure to obtain for them an impartial hearing.

REVIEW III.

1. *Traité Théorique et Pratique des Maladies de l'Oreille et des Organes de l'Audition.* Par le Docteur J. P. BONNAFONT.

A Theoretical and Practical Treatise on Diseases of the Ear and of the Organs of Hearing. By Dr. J. P. BONNAFONT. pp. 665. *Paris,* 1860.

2. *The Diseases of the Ear, their Diagnosis and Treatment; a Text Book of Aural Surgery, in the form of Academical Lectures.* By Dr. ANTON VON TROELTSCH (translated from the German, and edited by Dr. ST. JOHN ROOSA). pp. 254. *New York,* 1864.

3. *A Vindication of the Present State of Aural Surgery.* By a Member of the New Sydenham Society. pp. 43. *London,* 1864.

4. *On a New Method of applying Remedial Agents to the Cavity of the Tympanum.* By Dr. EDWARD BISHOP. pp. 19. *London,* 1866.

5. *Ohrenkrankheiten und Ohrenärzte in England und Deutschland; Ein Nachtrag zur Ohrenheilkunde der Gegenwart.* VON DR. W. KRAMER.

Diseases of the Ear and Aurists in England and Germany; a Supplement to the Aural Surgery of the present day. By Dr. W. KRAMER. pp. 96. *Berlin,* 1865.

DURING the five years that have elapsed since the important subject now under consideration was discussed in the pages of this Review, the pathology and treatment of ear diseases have not been quite at a standstill. Contributions have appeared from time to time in different periodicals at home and abroad, and one or two books of considerable merit have been published, in which Eustachian catheterism occupies a much more important position than it has heretofore held. It is true that the strides made in this department of medical science by the praiseworthy industry of one man, Toynbee, had been so rapid, considering how short a time the study had been conducted on a really scientific basis, that recent advance could scarcely be expected to keep pace with the progress of the preceding decennium; but there are not wanting earnest workers, such as Von Troeltsch, Politzer and Voltolini, who have been, and are now, collecting facts, and who are bent upon extending the field of aural pathology in its best and widest signification, *i. e.* a knowledge of ear diseases, founded on careful post-mortem examination, following honest clinical investigation.

For examination of the external meatus and membrana tympani, the tubular speculum, illuminated by reflected light from a concave mirror, such as is employed in laryngoscopy, is now in general use among the best aurists. If such a mirror be tolerably powerful, sunlight can be dispensed with, and clear daylight suffices; while there is this further advantage, that by fixing the mirror on a spectacle frame, or fastening it to the forehead by an elastic band, the hands can be left free for operation. Gruber's tubular metallic speculum, introduced by Wilde, is the one commonly employed, but a very nice dark speculum, made of caoutchouc, hardened by a peculiar process, is now manufactured and sold by Leiter, of Vienna, and is used by Politzer in his ear-clinique. This speculum has the advantage of cheapness, and, though perfectly firm, is not quite so rigid and unyielding to the walls of the meatus as the metallic instrument. Bonnafont employs by preference artificial light, and has invented an ingenious instrument for thus examining the meatus; but his method is not so simple or so practicable as the above.

The same author has a very good chapter on *polypi*. He, like Toynbee, has found them attached most commonly to the dermoid layer of the meatus, but sometimes springing from the membrana tympani itself. Bonnafont lays great stress on the importance of ascertaining accurately the seat and shape of the pedicle before attempting removal. Thus, if the polype spring from any part of the meatus externus, and have a narrow pedicle, it may be simply removed by torsion. If it be implanted in the membrana tympani, it must be removed by ligature. An instrument invented by him for this purpose, and figured in his book, is not equal to Wilde's snare, which is now in general use in England and Germany. In other cases where the base is broader and tough, he excises the polype, seizing it first with a fine double hook, and cutting through the base with a very delicate, slightly curved, bistoury.

The anatomy and pathology of the *membrana tympani*, which have been so fully worked out by Toynbee, and so abundantly illustrated in his museum (see Toynbee's Catalogue, series iii, prep. 107 *et seq.*), have been recently the subject of fresh investigation by Politzer (see 'Zeitschrift für praktische Heilkunde,' 1862, 46 and 51). The opacities of this membrane he divides into general and partial. The *general opacity* may be a consequence of disease either in the mucous or dermoid layer of the membrane, the former more often than the latter, owing to the greater frequency of inflammation in the tympanum. Thus, in a case of acute inflammation of the lining membrane of the tympanum, the membrane often appears of a bluish-red colour, which, as the disease progresses and becomes more chronic, changes to a bluish-white, and the whole becomes dull and turbid like a glass that has contained milk. The little triangular cone of light loses its brilliancy, and looks as if washed out (verwaschen), while the handle of the malleus is often drawn so forcibly inwards by thickening and retraction of the mucous membrane which covers the tendon of the tensor tympani, that it appears much shortened in perspective, or is rendered altogether invisible. This form of opacity is occasionally, though less often, limited to the fibrous layer of the membrane. The *partial opacity*, on the contrary, has its original seat most often in this very fibrous layer, from whence it extends outwards or inwards to the dermoid or mucous layer. It sometimes happens that, in cases of oft-recurring catarrh of the tympanum, which are not followed by perforation of the membrana tympani and purulent discharge from the meatus, small purulent deposits appear in the form of yellowish-white spots of various sizes scattered about the membrane, which do not clear away as the disease subsides, but remain to form dirty-white partial opacities. Lastly, the grayish-yellow or dull white, sharply defined patches, caused by lime deposit in the membrane, are well known to aurists, and are to be regarded as products of inflammation of the fibrous layer that have undergone the calcareous degeneration. Toynbee's artificial membrane still keeps up its reputation for being a most important aid in cases where the membrana tympani is perforated. Toynbee supposes that it acts simply by closing the hole in the perforated membrane. Erhard, on the contrary (über Schwerhörigkeit heilbar durch Druck, 1856), does not consider the stopping of the hole to be of any importance, but believes the improvement in hearing to be caused, just as in the case of Yearsley's bit of cotton wool, by the pressure which the foreign body exercises through the membrani tympani on the chain of bones. The same inflammation which perforates the tympanum will cause one of the ossicula to become separated from the other, as the incus from the stapes; and the artificial membrane, or the bit of cotton wool, acts by pressing them together, and so completing the chain for the conduction of sound. Von Troeltsch shows pretty conclusively that this is the correct view of the case. That the improvement in hearing does not depend on closure of the perforation, he has proved by stopping the hole with collodion without producing any improvement in the hearing, and then introducing Toynbee's artificial membrane with the greatest benefit.

Perforation of the membrana by operation is very favorably spoken of by Bonnafont. This operation, it will be remembered, is recommended by Toynbee in cases where there is deafness from complete closure of the Eustachian tube. Bonnafont makes an opening in all cases of chronic thickening with hardness and rigidity of the membrane. The difficulty of keeping patent such an artificial opening is very great. If bored with caustic it does not heal so rapidly as if made with a cutting instrument, but in both cases it will sooner or later heal. The immediate effect of this operation is often as striking as that of a successful operation for cataract.

A short summary of the different methods in vogue for examining into the condition of the Eustachian tube and the interior of the tympanum may not be uninteresting. 1st. The simplest plan is that of Toynbee, which is based upon the discovery made by that observer that, during each act of swallowing, the Eustachian tube is opened by the action of the tensor and levator palati muscles, so that mucus may escape from, and air enter, the cavity of the tympanum. By employing the otoscope, an elastic tube tipped at each end with ivory, one end of which is placed in the meatus externus of the patient, and the other in that of the surgeon, air can be heard entering the tube with a slight crackling sound at each act of swallowing, provided the tube be pervious. It may here, too, be mentioned, that Lucæ ('Archiv. für Ohrenheilkunde,' hft. 2) and Schwartze have shown that, not only during the act of swallowing, but also during every inspiration and expiration, there is a slight opening and closing of the tube, as indicated by a very slight falling in of the membrane at each inspiration, and bulging out of the same at expiration. The above method is insufficient, and is interesting physiologically rather than a trustworthy aid to diagnosis. 2nd. The method of Valsalva consists in the forcible inflation of air into his own tympanum by the patient, the mouth and nostrils being closed. If this be done and the tube be free, a sound or murmur, described by Wilde as "a sort of thug," is heard by the surgeon through the otoscope. This method has its objections, that many people cannot properly perform this act of inflation; and further, that in some cases where it fails, the tube is found to be really pervious when inflation through the catheter is practised. 3rd. As an improvement on the last plan, and, as a substitute for the catheter when patients are timid, Politzer has suggested the following, which is a kind of forced Valsalvian method, the air being inflated by the surgeon instead of the patient. A good-sized metal or caoutchouc tube is introduced about half an inch into the nostril, which it should exactly fill, and with the tube is connected a large pyriform caoutchouc ball or air-press. The person examined is then ordered to take a little water into the mouth and close the other nostril. At the moment that the water is swallowed, and consequently the Eustachian tube opened, air is forcibly pressed from the ball into the nose, and can, if the tuba be free, be felt by the patient rushing into the tympanum, while the membrana tympani may be seen at the same instant to bulge out. The otoscope is, of course, used in this just as in the Valsalvian experiment. 4th. The conviction seems yearly to be gaining ground in this country that *catheterism of the Eustachian tube* is a most valuable and often indispensable means of diagnosis, as well as treatment, in very many cases of deafness. Toynbee's disbelief in it has caused it for a long time to be looked at by the English aurists with suspicion, even though it has been for many years in constant use among the French and Germans; and though we do not, with Kramer, wish to overrate its value, yet we would advise all students to seek for an opportunity of learning how to use it. Although the above methods are practically useful, and will in most cases tell us whether the tuba is or is not free, yet they give no positive guide to the nature and seat of the obstruction, which can only be clearly ascertained by the catheter. Before mentioning the various uses which this instrument subserves in the diagnosis and treatment of deafness, a short account of the investigations recently made in Germany to determine its real effect may be acceptable. Kramer, of Berlin, from a series of observations made on the dead subject, and with an instrument designed by himself to imitate the construction of the ear, draws the following conclusions. Elastic and catgut bougies can, he asserts, be easily pushed through a metallic catheter right up into the tympanum, and the seat of strictures in the tuba can, by their means, be accurately determined. Air blown through a metallic catheter into the tuba does not itself pass into or circulate in the tympanum, but only gets as far as the end of the trumpet-shaped opening of the tuba, above which point it merely pushes onwards before it the column of air already present in the tuba and tympanum, and forces that air against the walls of the tympanum. If, however, an elastic catheter, which can be thrust farther into the tuba, be used in place of a metallic one, the inflated air does actually pass into and circulate in the tympanum. It follows that a murmur heard with the ostocope, when air is inflated through a metallic catheter, has its real seat in the orifice of the Eustachian tube; that, if this murmur sounds close and penetrating, it is a sign that there is no impediment in the tuba or tympanum to the conduction of the sound produced in the orifice of the tuba; but that, if it be distant and dull, there is an impediment in the tuba or tympanum, or both. On the other hand, air blown through an elastic catheter gives rise to a murmur in the very cavity of the tympanum. Much of this is flatly contradicted by Schwartze ('Deutsche Klinik,' 1863, 6—10), who has likewise experimented on the dead subject, and convinced himself that air can be forced through a metallic catheter right into the cavity of the tympanum and made to circulate there; also, that the murmur heard through the otoscope is really produced in the cavity of the tympanum. The error involved in Kramer's statement depends on the fact that he employed a catheter with too large a curve. The silver catheter employed by Schwartze has a smaller curve, and, through it, air as well as fluids can be injected right into the tympanum.

We may conclude, therefore, that as an aid to diagnosis, inflation of air through the silver catheter is most valuable, nay necessary, in all doubtful cases; that by the absence of any sound at all, or by its nature if present, by its proximity to, or distance from the ausculting ear, and by its dry or moist character, a practised ear can decide, with tolerable accuracy, not only the presence or absence of stricture in the Eustachian tube, but also the condition of the mucous membrane of the tympanum. For an accurate diagnosis of the latter, the history and general symptoms of the patient, as well as the appearance of the membrana tympani (on which Von Troeltsch lays great stress) must of course be likewise taken into account. Lastly, the catheter, as a means of treatment, can no longer be doubted. It is essential in all cases of stricture of the tuba, which is shown by Kramer to be, when properly dilated with graduated bougies of different sizes, as amenable to treatment as is stricture of the urethra. How far the inflation of different solutions, or of different vapours (as the lotions in pulverised form recommended by Bishop, or vapour of tar by Bonnafont), can be said to be of real therapeutic value is very questionable.

Von Troeltsch makes the surprising statement that out of forty-eight children (from seventeen hours to one year old) examined at hazard, he found the middle ear healthy only in thirteen. In the remaining thirty-five the lining membrane was congested, and the cavity of the tympanum filled with pus, while small red bodies, varying in size from that of a small pin's head to that of a hemp seed, hard, and composed of a rich vascular cortex with granular or cellular contents, were found in many cases attached to the mucous membrane. He insists, therefore, and with reason, on the frequency of purulent catarrh in children, and draws attention to the fact that the disease is very often overlooked. Loud cries of pain from a child, in whom slight fever is present, and in whom there is no evidence of abdominal disease, ought always to excite a suspicion of inflammation in some part of the ear.

The term *nervous deafness* is now employed by aurists to signify deafness which cannot be accounted for by disease in the outer or middle ear, or in the tuba. The diagnosis of such a condition must, therefore, be strictly negative, and its frequency will depend partly on the capabilities and appliances of the surgeon, but not a little on his fancy. Nervous deafness is well defined by Von Troeltsch to be "that condition in which the patient cannot hear and the surgeon cannot see," just as formerly amaurosis was defined as "an affection of the eye in which neither patient nor surgeon can see." Kramer, who formerly estimated cases of nervous deafness at rather more than one half of all cases of deafness that occur, found his powers of diagnosis so marvellously increase, that in the course of a few years he was able to reduce the number to 4 in 1000. Here he has certainly erred on the other side. Toynbee's specimens show that affections of the internal ear are much more common than this. Again, Voltolini states that in nearly all the temporal bones of deaf persons examined by him he has found morbid changes in the internal ear. Bonnafont and others lay great stress on the application of the watch to different parts of the head, especially the parieto-temporal region, as a positive aid to diagnosis of disease in the internal ear. If the watch be heard when thus applied, even though there be complete deafness, you have an indication that the auditory nerve is unaffected, the sound being conducted to it by the bones of the skull, and that the disease is seated either in the external or middle ear, or in the tuba. On the other hand, if the watch be not so heard, it is certain that the auditory nerve is affected, and the case must be looked upon as hopeless. A watch is better for this experiment than a tuning-fork, in that the vibrations of the latter may seem to be *heard*, and yet are really only *felt*; many, even intelligent people, being unable to distinguish between feeling and hearing vibrations under such circumstances.

M. Bonnafont's great book shows much careful work and original research, but is too long and too prolix to find much favour with practitioners or students.

Von Troeltsch's text-book is the best modern book on ear-diseases with which we are acquainted, in that, while giving due prominence to the value of the catheter in the diagnosis and treatment of deafness, it still looks on careful post-mortem observation of cases as the essential part of an aurist's work, and lays proper stress on constitutional treatment as a most important part of an aurist's therapeutics. The English translation is not worthy of the book. It abounds in ugly and ungrammatical sentences, and is more difficult reading to any Englishman with a tolerable knowledge of German than the original.

The 'Vindication of the Present State of Aural Surgery,' is partly an attack on the New Sydenham Society for having translated Kramer's work, and for having put that book before the English medical public as a fair representation of the aural surgery of the present day; partly also, an *exposé* of the errors that the book contains. To this the Sydenham Society would probably answer, that it is not every year that such first-rate books as Frerich's 'On the Liver,' or others, which they have so well supplied us with, can be found for translation, and that, failing these diamonds of the first water, they are obliged to select the best they can from an inferior quality.

It may well be said of Dr. Kramer, that "his hand is against everybody." His pamphlet, under notice, has been written to show that all aurists in Germany and England are ignorant and fools, and that he only, Kramer, is knowing and wise. At the head of the long list of these ignorant men—the aurists in England and Germany — stands, of course, the name of Toynbee. His attack on this famous aurist is but a repetition of what he has already published. The statement which he makes and reiterates, that "Toynbee did not see or examine during the lifetime of the patient one single case of the 1659 specimens collected in his museum and tabulated in his published catalogue," might formerly have been attributed to ignorance of the English language, and so, in a sense, excusable; but can now only be regarded as a deliberate falsification. Dr. Kramer has lived and practised in England, and he has corresponded in our language. It has been already pointed out to him, that in the 'Medico-Chirurgical Transactions,' vol. xxxviii, Toynbee, in writing of the cases in his museum, states that he has himself examined, during life, 136 of those cases. Besides this, if Dr. Kramer will refer to pages 107, 154, and 587 of the 'Medical Times and Gazette' for 1855, he will find detailed histories, related by Toynbee, of cases which he has himself treated, and from which the specimens contained in his museum, and described in the very catalogue at which Dr. Kramer so unjustly sneers, are taken. The younger German aurists, as Voltolini and Politzer, fare no better than Toynbee: their principal offence, in Dr. Kramer's eyes, being that they have faith in Toynbee, and that they are striving to further the knowledge of their subject by careful post-mortem observation added to clinical study. It would be well for all, if the older aurist were to cease henceforth from calumny and controversy, and follow the good example that his juniors are everywhere setting him in Germany.

REVIEW IV.

Surgical Experiences: the Substance of Clinical Lectures. By SAMUEL SOLLY, F.R.S., Senior Surgeon to St. Thomas's Hospital; Member of the Council, and late Professor of Anatomy and Surgery, in the Royal College of Surgeons of England, &c. *London:* Hardwicke, 1865. 8vo. Pp. 656.

MR. SOLLY is already well known to the medical profession as a writer. His work on the human brain takes a high place among treatises on that subject, and has earned for its author a large measure of approbation. It shows an amount of physiological, medical, and surgical knowledge which is not often combined in the same individual, and above all it displays a thoughtful, observant, and discerning mind. The same characteristics are visible in the volume before us; although the author has hardly so fine a field for their exercise as he had in his work on the brain. In a course of clinical lectures no subject can be treated very fully or at any great length; still the present volume can hardly fail to add to Mr. Solly's reputation; for it gives ample proofs of his insight into the hidden sources of disease, and of the originality and skill which he shows in devising the means of treatment.

We are glad when a surgeon of ripe age and large experience publishes a work like this. It can hardly fail that a man who has been connected with one of the largest metropolitan hospitals for thirty or forty years has something valuable to say to his professional brethren—a legacy to leave them which shall be worthy of their acceptance. It is almost certain that a man in such a position will have had his attention drawn to some particular subject in surgery, or that he will have seen rare and exceptional cases; and these, detailed with the knowledge which is afforded by the study and experience of a life-time must be instructive to those who come after him.

Mr. Solly's book is in the form of clinical lectures. In the preface a list of his dressers is given, and the volume appears to have been compiled in the first instance for their use, to recal to their minds the instructions they have received from the author, and the cases they have seen in the now demolished wards of old St. Thomas's Hospital. For general professional readers it seems to us that the book might have been considerably curtailed with advantage. The cases are often reported at unnecessary length, and the lecturer's remarks are occasionally occupied with elementary matters, which might, perhaps, be taken for granted.

The subjects which engage Mr. Solly's attention are those which would naturally form the basis of clinical teaching in a medical school, and include almost all the most important topics in surgery. The author's style is easy and colloquial; and we have noticed with pleasure many excellent hints to students on their conduct towards their patients scattered throughout these lectures. In a few instances the clearness which pervades the book is obscured by the introduction of too many cases—case within case—so that we are apt to forget the leading case, which forms the subject of the lecture, in the secondary cases which are inserted by way of illustration.

Mr. Solly's experience extends over a very interesting period in the history of surgery. Forty years ago blood-letting and mercury were still at the height of their reputation. They were in constant use and their value was scarcely questioned. But a great change has taken place since then. In London practice, at any rate, blood-letting is rare, and mercurial salivation is almost regarded as an unmixed evil. The same change which has passed over the profession as a whole has no doubt taken place in the mind of our author, and we think that we can distinctly trace it in his lectures.

During the earlier part of his career he was evidently much more in favour of strong measures than he has been of late years. In fact it is interesting to observe how he has come to feel more and more the insufficiency of the surgical art and to depend more and more upon the efforts of nature. And does not this also represent a change which has been going on in the profession at large? Not only have we lost a great part of our confidence in bleeding and mercurial salivation, but we have learnt that our first business is to put nature in the most favorable situation, and then only to interfere when it seems certain that we can do some good. In proportion as we have become distrustful of our own severe measures, we have learnt to trust to the simple efforts of nature. To detect the earliest symptoms of disease, to discriminate between different varieties, and then to apply the simplest remedy and to persevere carefully in the use of it—this has been the tendency of our recent progress, and this is the direction in which we must look for the advancement of our art. Operations will always be necessary, but they will become rarer and rarer as surgery approaches perfection; and this appears to be the view of the case which our author takes. We are glad to see that he lays great stress on the medical treatment of surgical cases, a point which is too often neglected by surgeons. He enters fully into the details of every case with a view to making an accurate diagnosis, and he then adopts the simplest and most natural methods of treatment, both medical and surgical.

Having said thus much as to the general scope and tendency of Mr. Solly's experiences, we shall now lay before our readers a few extracts which will serve to show the many interesting topics with which our author deals.

In speaking of blood-letting, and in pointing out those cases where he believes it may be employed with great advantage, Mr. Solly gives the following sketch, drawn from his own personal experience when he was a student.

> "One of the earliest cases which convinced me of the value of blood-letting in the treatment of severe internal injury was my own. About the year 1825, when I was an apprentice of Mr. Travers, I was severely injured in the hunting field by a large and heavy horse falling across the lower part of my chest, on the left side. I was not stunned, but felt as if all the wind was knocked out of me, and believed that three or four of my ribs must be broken. I was carried into a neighbouring farm-house, and as soon as the doctor could be found, I was bled. I was afterwards placed in a post-chaise, and supported by two friends, one on each side, I rode about five miles, to Dunmow, in Essex. During the ride I twice expectorated a small quantity of blood. It was not pleasant to look at, as I imagined that the lungs must be lacerated. It did not recur, so I had the satisfaction of finding that my fears on that point were groundless. I was confined to my bed for a week, and during that time lost seventy ounces of blood, by the lancet and leeches.
>
> "My symptoms indicated inflammation, both of the left pleura and the left side of the abdomen; and from frequent spasms of the diaphragm, which were frightfully painful, I thought there must have been some slight laceration of its fibres. I got well enough to return to my studies at the hospital in about a month, but I did not quite lose the pain on the left side for six weeks or two months. I never felt the slightest ill-effects from the blood-letting, but great relief in every respect. I shall always think that my life was saved by it." (P. 138.)

The injuries of the head and spinal cord form a subject which Mr. Solly writes upon *con amore*. It carries him back to the days when he was preparing his work on the human brain; and the recollection of his labours on that subject cannot fail to add weight to the following theory, concerning the nature of *determination of blood to the head,* and the mode of its production.

> "The expression, 'determination of blood to the head,' is often made use of, but without any explanation of the manner in which this takes place. I doubt whether the profession generally have any distinct idea as to the exact condition of the vascular system that produces it. I would venture to offer the following theory. . . . It applies, not merely to the head, but everywhere else.
>
> "The middle, or muscular coat of the arteries, in a state of health, contracts with each systole of the ventricles, just sufficiently to give solidity to the wall of the pipe, so that the force of the contraction is not lost on a yielding surface; a much greater force is required to drive water though a leather hose than through a leaden tube. The middle coat contrasts sufficiently to assimilate the artery, physically and temporarily, to the leaden tube. Arteries with permanently rigid walls, like leaden tubes, would have interfered, by their rigidity, with the motions of the limbs; and hence this beautiful contrivance. When this middle coat does not contract, or only contracts imperfectly, then the force of the heart dilates the tubes, and produces congestion.
>
> "I believe, then, that determination of blood to the head arises simply from deficient contraction of the muscular coat of the capillaries of the brain, preceded by excitement of the heart's action." (P. 169.)

There is hardly any operation which has given rise to more discussion of late years, than excision of the knee-joint. Mr. Solly's opinion on this subject will carry with it the more weight, becauses he frankly confesses that at first he opposed the operation, but afterwards he became convinced of its value. The reasons which induced him to change his mind are given in the following paragraph,—

> "When this operation (excision of the knee-joint) was reintroduced by Mr. Fergusson in 1830, I confess I was veryincredulous as to its value. 1 thought that those cases that would be cured by excision might be cured without it. I had had, as you have heard me say before, several cases of apparently hopeless disease of this joint arrested by the repeated use of the moxa externally and cod-liver oil within a good, firm; anchylosis being the result. On the other hand, I have had cases of ulceration of the cartilages which have been carried off by phthisis during the progress of a local cure. But we have yet no proof that either excision or amputation would have averted such an untoward result.
>
> "The facts, however, that have converted me to the belief that excision of the knee-joint is not merely admissible in many cases, but

that it is decidedly desirable, are: First. The time occupied in procuring a perfect cure by anchylosis, which ranges from two to four years in all adults. Secondly. That even in the most favourable cases there is great tendency to a recurrence of the disease. And thirdly, that the statistics of the cases already published show that the operation is not so fatal as amputation of the thigh." (P. 283.)

It is often asked, how long a piece of dead bone, if left to itself, will take to separate. Here is a case which may perhaps help to answer the question, though we fear the answer which it gives would be far from satisfactory to the patient.

"When you consider the small opening to the bony prison-house in which the dead bone was confined, and the thickness of the prison walls, you will comprehend the value of the operation from the difficulty the sequestrum would have had in making its escape. Months, nay years, might have elapsed before this would have been accomplished by unassisted nature. I have just had before me some papers for an insurance office, in which mention is made of exfoliation of the lower part of the thigh, which has been going on now for about thirty years. The individual is now about thirty-nine years of age, and the disease was induced by a fracture when he was a child. It is in such lingering cases as these that you should advise an operation, or, at least, a thorough exploration under chloroform. It is true that patients will not always submit, and that the case drags on, contrary to the advice of the medical adviser; but still he has done his duty." (P. 307.)

Stricture of the urethra is a disease of such frequency, of such difficulty, and of such importance, that good advice respecting its treatment is always acceptable; and Mr. Solly's advice on this subject appears to us to be particularly sound and useful.

"I hope that the relation of these cases will encourage you to avoid the use of the knife in the treatment of stricture. I am quite as ready to use the knife in surgery as any man, if I think the case requires it; but I am quite sure that, as a rule, we ought to avoid its use when other means will attain the same end. The longer you practise your profession the more will this principle be impressed upon your minds. There is another thing to be remembered in the treatment of stricture: never be ashamed to leave the bedside of a patient without succeeding in passing a bougie. The late Mr. Copeland, who was as sound and practical a surgeon as any in his day, said to a patient who afterwards came under my care with a very irritable stricture and false passage, 'Mind you never put yourself under the care of a surgeon who is afraid to acknowledge that he cannot always succeed in passing a catheter.' I am told that a hospital surgeon, now deceased, passed a sleepless night from vexation if he failed to introduce an instrument into the bladder in the presence of his pupils. Such a man must have made many a false passage. Of such a proceeding you should have the most intense horror. Every good surgeon will fail occasionally in the introduction of a bougie; but no good surgeon will make a false passage, though a skilful surgeon will sometimes do it, when his temper or his pride rules his hand, instead of his reason and his conscience." (P. 536.)

These extracts will serve to show our readers the style in which Mr. Solly writes. But we cannot, by means of extracts, do justice to the most important parts of the volume before us. It must be read *in extenso* to be appreciated. The chapters upon injuries of the head and upon the diseases of bones and joints are specially worthy of attention. But the most interesting part of the whole book is that in which the author treats of the disease which is commonly known as *Scriveners' Palsy*. Mr. Solly has had his particular attention directed to this subject for a length of time, and he has embodied the result of his experience in this work. Several examples are detailed, and the pathology of this curious complaint is fully discussed. Our author holds, that it consists essentially in a degeneration of the cervical enlargement of the spinal cord, the result of excessive and long-continued use of those muscles which are employed in the act of writing. The theory of a central origin of the disease is ably worked out, and deserves careful consideration. In support of his views our author quotes the following case, which he holds to be exactly analogous to Scriveners' palsy, only that the effect was produced suddenly instead of gradually. With this case we shall bring our notice of this interesting and instructive volume to a close.

"The following is one of the most striking cases of paralysis I know of, induced suddenly by little more than an hour's continued strain upon a nervous centre, confirming all that I have said regarding the controlling and co-ordinating power of the spinal cord.—A gentleman got into the five o'clock express train for Brighton. He was no sooner seated than he felt a strong desire to have the bowels relieved. But there was no help for it; the train started, nor did it stop till it reached its destination, at a quarter past six o'clock. All this time the desire to go to stool increased more and more, but he restrained it successfully by a violent effort. The moment the train stopped he sprang out of the carriage, when the sphincter ani relaxed, and the contents of the bowels were poured out into his clothes. The curious part of the case is that he became paraplegic. The spinal cord had been over-exerted, exhausted, and paralysed by the long-continued effort.

"In this case, the brain, from which, I suppose, all must admit that the will emanated by which he desired to control his sphincter, did not give way. His intellect was not impaired, and the whole muscular system was not paralysed—only that portion which is supplied by the lower portion of the cord. I regard this as identical in its pathology with Scrivener's palsy, only that the destruction of the nervous centre was sudden, not gradual. I do not know what treatment was adopted in this instance, but, reasoning from analogy, I should have advised complete rest; that some arrangement should have been made to prevent his ever having to control the action of the bowels, if he had in any degree retained that power." (P. 231).

REVIEW V.

A System of Medicine. Edited by J. RUSSELL REYNOLDS, M.D., F.R.C.P. Eng. Vol. I. Containing General Diseases. *London:* Macmillan and Co. 1866. pp. 952.

WE cannot too often express our obligations to those labourers in the cause of medicine who, from time to time, take stock of the common capital, and help us to survey comprehensively our position, both as respects knowledge and its application to our daily tasks. The necessity for a certain degree of harmony between the several parts of such a work is the best antidote to the evils of specialism, which it may render not only harmless but decidedly nutritious to the body politic of our profession. And one such stock-taking at periodical intervals is not enough, for in point of fact there are several ways of doing it, which cannot well be united without sacrificing much of the peculiar advantages of each. One form is that of a vast encyclopædic collection of all that has been said or done at all ages by all that have advanced our science or art; and if the author takes his time about it, as Dr. Copland has done, he may produce a κτῆμα ἐς ἀεὶ which will be long without a rival. As it is requisite that each subject should be treated exhaustively, the completest way to avoid omissions and repetitions is for the whole to be the progeny of one pen.

Another form is that of which the most recent example is the work of Dr. Aitken, 'On the Science and Practice of Medicine,' in which the latest good writers on each subject are taken as the representatives of their class, and their opinions and observations are melted into one, as illustrations of the time-honoured doctrines on which medicine is based. This should be done, as it is in the instance quoted, by a professor who is exempted from the responsibilities, and consequent strong predilections, of daily practice, and who has leisure for calm comparison and review. It should be accompanied, as we hope in a future edition Dr. Aitken's book will be, by full-length references to the authors, so as to afford facilities to those who wish to form an opinion for themselves on the questions mooted. It is, for example, useless to give a string of names, such as "Gierse, Roger, Von Bærensprung, Wünderlich, Friedlander, Virchow, Traube, Jockmann, and others" (p. 29, vol. i), without telling the overpowered reader where he shall find their writings.

Another form, of which 'Dr. Watson's Lectures' have long been the deservedly popular representative, exhibits the effect on the practice and principles of one thoughtful man which the collection of knowledge up to the present time produces.

Of these contributions to literature each is valuable in its way. The first is an indispensable book of reference for one who wishes to compare what men of mark have thought on any given matter under various mental influences; the second shows how much substantial agreement there is between honest cultivators of rational medicine, in spite of the apparent discrepancies which its enemies delight to make the most of; the third exhibits a model which, when the mind of the reader is of the same order as that of the writer, is a comfortable and safe guide. Dr. Reynolds' aim differs from all three. He does not affect to rival the all-embracing learning of the first, the scholastic eclecticism of the second, or the refined simplicity of the third; but he would make the practitioner his own eclectic, and lead him to elaborate his own ideal. He would present him in his study at home with such an idea of the best progressive practice in the current year as he might gain by being in twenty different places at once during the twelve months, studiously watching the effects on variously educated minds of the facts elicited by recent science; studying, in fact, the orthopraxy deduced from the orthodoxy of the generation in its prime. We do not mean to say that the editor puts forth in black and white such an intention, we infer it from his general tone and his *acta non verba*. To this end he would have his readers learn, from actual workers themselves, their own most recent views, not, indeed, on all subjects, but wherever they have modified diagnosis or treatment. The contributors are, in some instances, those who have already established an European reputation by their labours on special subjects; but more often Dr. Reynolds has, we think, wisely, assigned the parts, not to those known to the world as the parents of original views, but known to him as successful cultivators and critics of them. He thus avoids the risk of "viewiness," of looking at general medicine as if from out of the window of one study after another.

The same plan of united labour was adopted in the 'Cyclopædia of Practical Medicine,' completed in 1835; and in the 'Library of Practical Medicine,' published in 1840; and the quarter of a century's use which these books have had will enable their successor to learn what in them is to be avoided and what worthy to be followed. That same quarter of a century has greatly changed opinions and practice, perhaps more than any other quarter of a century of which we have authentic records. The works named may be fairly stated to depict the application to practice of the then dominant anatomical science, which had added in its time so much to our professional knowledge. The physiology which now guides us was then but germinating, or, at all events, no attempts had been made to ripen its fruit into practical utility. If any doubt the revolution in the art of medicine which has been thereon consequent, let him try for a single week to follow the prescriptions guaranteed by respectable names in the 'Library of Practical Medicine,' and then say whether he does not think that a new representative of our present position is urgently required.

Dr. Reynolds has discarded the alphabetical arrangement of subjects used in the 'Cyclopædia,' and taken one more resembling that of the 'Library.' He would have the book read through, not made use of merely for reference; and he adopts a natural, almost clinical, order in which to place the matter.

"It is proposed to make the first division of diseases into their two great groups: 1st. Those in which the whole organism appears primarily and prominently deranged; and 2nd. Those in which special organs or systems of organs are, in like manner, affected. Subdividing the first group, we have two classes: A, those in which the disease appears to be developed by causes operating from outside the body; and B, those in which the malady seems to depend upon some internal change. Thus, in the first subdivision we find the acute specific diseases and their analogous affections; in the second, gout, rheumatism, scorbutus, and the like. Subdividing the second group, we have many classes, consisting of diseases of systems of organs, such as—A, diseases of the cutaneous system; B, diseases of the nervous system; C, diseases of the circulatory system; D, diseases of the respiratory system; E, diseases of the digestive system and its appendages; F, diseases of the urinary system; G, diseases of the reproductive system; and H, diseases of the locomotive system.

"Each of these is, in its turn, again subdivided, upon the primary principle of general or partial change, so that, in regard of the nervous system, for example, we have—1st, those of general or undetermined seat; and 2nd, those depending upon distinct local change in its parts, anatomically considered: and this leads to further reduction into affection of parts, such as—A, brain; B, spine; C, nerves; whereas the final division is based upon the nature of the changes which these portions of systems undergo.

"If this mode of arranging diseases has no other merit, it has that of simplicity; and it will, we believe, bring, as a general rule, into closer proximity than some more ambitious systems would allow, those diseases which have the most intimate clinical association. It involves little theory in any case; none in many; and may, therefore, commend itself to those who realise, amid the great progressive science of medicine, the difficulties and dangers which attend upon all nosologies which, based on theories, partly right and partly wrong, carry with them, and only with great effort disentangle themselves from, what is erroneous in their groundwork and *à fortiori* luxuriant in their after-growth."

This volume is occupied by the first group, on "General Diseases," and begins with an "Introduction," from the pen of the editor, in which are discussed briefly the generalities concerning life and death, health and disease, the force of terms such as "structural," "functional," "predisposition," "exciting cause," "symptom," &c., the logic of diagnosis, the functions of pathological anatomy, and especially its bearing on therapeutics, the recent hygiènic tendency of the healing art, and the relation of theory to it. As respects the connection of medicine as a science with medicine as an art, what Dr. Reynolds would have the practitioner of the day act upon is, not traditional empiricism, but that better knowledge of the laws of vital and morbid changes which are now possible. And he is very hopeful that such is, in fact, become the basis of our practice. As he sanguinely says—

> "We see that the man is greater than his maladies; that his general condition is of more importance than his local ailments; that disease is a change in him rather than in some parts of him: and that no treatment can be of any real service which sacrifices the greater to the lesser. In all treatment, therefore, what is general is to be dealt with upon the basis of a true appreciation of the general pathological condition, and this in spite of all theories in regard of local changes, however they may be termed, whether they come to us with names hoary with age, or scarcely intelligible, and sometimes even ludicrous, from their novelty. If the general condition be one of weakness, it matters not that the brain, the heart, or the lungs may be in the state of so-called "inflammation;" the weakness is the one thing that demands immediate treatment, and to neglect its treatment is to run the risk of sacrificing the patient to the theory of a compound state, even now but imperfectly understood. This is the starting-point, the essential element in therapeutics."

The reader may smile to see that Dr. Reynolds, in waging war with theories, wields a therapeutical theory of a most pronounced character. Far be it from us to blame him, for we are sure he has got hold of a weapon of excellent temper, though as yet rough and unground, which will soon make other arms ridiculous, with all their polish. It will be seen that his expectations from the medicine of to-day are much higher than those of Van Swieten and Boerhaave, the most philosophical physicians of their generation, who confessed that the whole aim of the art of their time was to diminish vital force,—"All that art can do is to weaken life."[1] We hope and believe this is becoming untrue.

The opinions of the editor have been first quoted and commented upon, because they may be presumed to afford a sort of guarantee that, without infringing on individuality, they will in some degree flavour the whole work. It is impossible, we should think, for any one to follow up these prefacial sentences by falling back upon the perturbative plan of treatment, taught always in words and often in practice a quarter of a century ago.

The editor has done well to open the ball with Dr. Parkes' article on "Influenza." It is a capital specimen, in whose twenty-two pages there is afforded more useful information in a readable form than is contained in many a heavy volume. The numerous full references in the notes and the epigrammatic turn of many a sentence, show how much selection and erasure have been used in the construction of this article. May we be allowed to remark, with pride, that this pen perfected its cunning by practice on the pages of the 'Medico-Chirurgical Review.'

In Dr. Maclean's treatise on "Malarial Fevers," which follows, we must say that, as English practitioners and reviewers, we could have wished some space had been spared for ague as seen in this country. A student from reading it would expect never to come across a case, except in India. We should have been glad, also, of more concerning the paludal fever of Southern Europe, with its tendency to local inflammations. In these days of locomotion the home practitioner has sometimes to treat relapses of it, and many a travelling physician would be glad of information about it in his own language, and seasoned with British common sense. Dr. Maclean is mistaken if he expects to find the jugulatorial treatment so fatal, among the Latin races at any rate, as he and Captain Burton represent it. "A. B— caught fever—gave him calomel, bled him, blistered him,—died on the third day." This is far from being the best treatment of fever, but still it is not so pernicious as, mayhap, it deserves to be. Patients are tougher than Dr. Maclean supposes; and bloods, certainly Italian bloods, will bear diminishing in quantity for the object of diverting local congestion. Of course a calculation must be made, as indeed it must in all treatments whatever, whether the play is worth the candle.

One is rather startled at finding Dr. Edward Goodeve's essay on "Diarrhœa" printed in this part of the work; but officially, diarrhœa is a "zymotic disease" of the "miasmatic order," and there would have been an awkwardness in giving a second article on the diarrhœas which the Registrar-General scorns to acknowledge, or on diarrhœa as a morbid act independent of its cause. So they are all clubbed together here, and the consequence is, that those forms which are out of place occupy the back seats, and India again comes prominently forward. This renders the treatise less satisfactory than it might otherwise have been.

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[1] Van Swieten's 'Commentary on Boerhaave,' I, § 106.

In "Dysentery" and "Cholera" this is, perhaps, as it should be. Indian experience has a right to the front row here. Still we are rather surprised that Dr. George Johnson's observations on cholera should be so utterly ignored, as not to be even named. Possibly they might modify the incisive formulæ which begin the treatment of several stages. "*Evacuation stage.*—The discharges should be checked, if possible. "CHOLERAIC DIARRHŒA.—The discharges should be restrained." It is just possible that treatment may have different results in England and India; and if so, we ought to have a few separate pages for the use of home practitioners; but certainly the "arrestive treatment" in all but the very earliest threatenings, seemed to those who had hospital experience during our last epidemic, injurious. Opium, unless it happened to have been given early enough in the attack to have been purged off, rendered the collapse more dangerous. We could wish, also, that a stronger caution had been here given against warm and vapour baths in collapse. Dr. Goodeve, indeed, says they "are of no use," but that will not prevent fussy people from applying them, to the patient's great torture and peril.

Dr. Bristowe handles fully the subject of "Pyæmia." We are glad to see that he draws the attention of the profession to the exceeding unsatisfactory nature of its treatment, in spite of the intimate knowledge of its pathology with which we have been made familiar by the labours of Mr. Lee and others. Even in respect of prophylaxis after wounds and operations, investigators seem satisfied with advice suitable only to public institutions, as if pyæmia never occurred in private practice. The anti-fermentive treatment of Professor Polli, by means of the sulphites, certainly deserves a trial.

Papers on "Mumps," "Hooping-cough," and "Croup," follow. Though they are not placed together, we should advise the last-named to be read along with that on "Diphtheria," by the same careful author, Mr. William Squire, in a later part of the volume. A point is made of the diagnostic anatomical distinction between croup and diphtheria, consisting not so much in the difference of the exudations, which Professor Virchow demonstrated to be more albuminous in the first, more fibrinous in the second, but yet exhibiting transitorial forms—not so much in this chemical character as in the condition of the surface on which they lie. This in croup is uninjured; in diphtheria it is superficially destroyed by ulceration or interstitial necrosis. When the croupous formation is peeled off, the mucous membrane appears only reddened; when the diphtheria is removed, it bleeds from lesion of continuity. (This difference has been pointed out by Schönlein, 'Pathologie und Therapie,' "*Angina membranacea,*" and "*Angina gangrænosa,*" vol. i.) The former is a disease of the nature of common inflammation, of which the fibro-albuminous false membrane is the right end; in the latter, the exudatory process is an accompaniment to, and an essential part of, the disease, but is not its termination. In fact, croup is a phlogosis, diphtheria a neuro-phlogosis. The marked way in which the distinction between the two is founded on the condition of the general system, producing the varieties both of sporadic cases and of epidemics, is highly important in a therapeutical point of view. Mr. Squire explains by it very appositely the discrepancies in the statistics of different modes of treatment.

"Syphilis" is brilliantly handled by an hospital surgical teacher, Mr. Hutchinson. And it is right that it should be so, for somehow, at all public institutions where there are pure surgeons, this disease has grown to be their property, and therefore, they, of course, have most experience. Even at the Lock Hospital, where there is both a physician and a consulting physician, they have no regular duties, and are only called in to non-syphilitic or extraordinary cases. The consequence is, we believe, a certain retardation in the pathology of the subject. Assuredly now for the first time we find syphilis in its right place, grouped as a specific zymotic with smallpox, measles, scarlatina, &c., differing from those acknowledged eruptive fevers less in its essential nature than in the duration of its stages. The apparent differences are by no means real ones, as Mr. Hutchinson shows by his parallelism of their contagion, incubation, efflorescence, relapses, decline, sequelæ, and partial preservative influence. But the fact that he should be obliged to do so in 1866 shows the general backwardness spoken of above. We heartily congratulate syphilis on its escape from the chronic Impetigines where Dr. Cullen buried it, and Dr. Tweedie still left it in 1840.

Perhaps some young physicians, proud of their "purity," may scorn paying attention to the subject. But we quite agree with Mr. Hutchinson, that "the power of recognising syphilitic diseases, when brought under notice, is one of the most valuable gifts a physician can possess." And we would add, that it is a much more difficult task to him than to a surgeon; for, as a rule, people go to the latter with a full acknowledgment of their disease, anxious to force it upon his notice, whereas their reason for selecting a physician is often that they may conceal it, frequently from a doubt of its nature.

Mr. Hutchinson does not decide upon several questions which have been often debated with more zeal than discretion, such as the unity of the virus, the perpetuity or eradicability of the disease, the treatment by inoculation. He wisely thinks the evidence on either side quite inconclusive. The latter subject, indeed, he does not mention; but as an opportunity is now being afforded by the Lock Hospital of testing its merits, we hope to see the results commented on in a second edition.

Between this subject and the exanthemata comes in, rather surprisingly, the "Plague." It is difficult to understand the reason for its divorce from the typhus group, especially as the author insists much on their relation.

The articles on the acute exanthemata give clearly and well the established information on their subjects. To Mr. Marson's on "Smallpox" is appended one by Dr. Seaton, the government adviser on the subject of "Vaccination," about which he writes. The most important of the recent questions regarding this preventive, viz., the allegation of the introduction of other diseases by its means, is fully gone into, and as accuracy of evidence is all-important on a controverted point, very full references are given.

The sheet on "Typhus," by Dr. Buchanan, physician to the London Fever Hospital, offers much condensed information, and yet is easy reading. That on "Enteric, or Typhoïd Fever," by Dr. John Harley, is more than double the length, and not so agreeably written, but it contains some original microscopic illustrations of the morbid anatomy of the disease. His view of its etiology is, that it is a consequence of the vitiation of the portal blood by a poison, somewhat resembling the narcotico-acrid principles of some plants, generated of organic matter, decomposing commonly outside, but possibly inside the body. This vitiated blood may be evacuated by the liver, but if not, it passes into the general circulation and causes the inflammation of the bowels and other symptoms which, in other cases, may be produced by other causes. We regret to see that he still allows hydrargyrum cum cretâ to form so considerable a part of his materia medica in the treatment, that he even continues its use till tenderness of the gums and mercurial fetor of the breath appear. Statistics, we believe, have thoroughly shown this treatment to increase the mortality, and rational pathology fairly explains why. He certainly compensates in some degree by accompanying it with quinine in intermitting cases, and still more by his recommendation of the plan of continuous nutrition in all; and he guards it with so many provisos that we expect he is on the point of reforming it altogether. Our hope in the decaying nature of his prescription is strengthened by the fact of several orthographical blunders appearing in it.

"Pulvis Cretæ Aromaticæ cum Opio, gr. v."

has the aspect of a neglected child "passing away."

The occasional character of epidemics which has been denominated "Relapsing Fever" is treated of by Dr. Warburton Begbie, whose name is a guarantee that it is treated judiciously.

That Mr. Denis Macdonald, to whom "Yellow Fever" is assigned, is a naval surgeon, we see evidence in the "stout glass of rum and water" which is so meritorious in his eyes; and that he is a good officer and a bit of an artist in the picturesque way he paints the disease, and pithily lays down the law on the subject, without, at the same time, any contempt for proper authority. We should have been glad of his experience, or, at least, his opinion on the use of quinine as a prophylactic.

We do not know whether Dr. Reynolds sent round his treatise on "Erysipelas" as a specimen of what he hoped to get from his collaborators. He might have done so with advantage. His is one of those minds so valuable to the literature of our profession, which is more struck by the importance of resemblances than of differences; and he points out how the same pathology extends over a great variety of typical forms, beginning at one end with erythema, and ending with diffuse cellulitis, and so passing into pyæmia. He does not say whether he intends to include erythema in his article, but we presume so, by his naming errors in diet, such as shell-fish, among the exciting causes. The other boundary has already been treated of. He believes that the class of cases which has been described in such a manner as to justify the use of antiphlogistic treatment exist only in history or imagination, and therefore it appears unnecessary to describe them, or to prescribe treatment for a patient, "provided he is found in a condition we never meet with."

He very properly deprives the "erratic" form of its supposed fatal character. We have ourselves seen it wander over a boy's whole body, from toe to top, attack the throat, exhibit itself as diarrhœa in its passage through the intestines, reappear at the anus, and again attack some of the lower parts, without the patient having ever been in the slightest danger. The risk must of old have been in the doctor.

The succeeding pages are on the diseases known to arise from a morbid poison communicated from the bodies of other animals to ours; "Glanders" and "Hydrophobia." They are from the pens of the Gamgees, *par nobile fratrum*, and are worthy of the name they bear.

> "As glanders is a disease which always originates in the horse and ass, never occurring in man except when communicated, directly or indirectly from them; and the nomenclature of the disease having been borrowed to a great extent from that previously in use among veterinary writers, its complete history, in the first place, necessitates an account of the mode in which it originates in the lower animals, and of the forms which it in them presents."

This sentence explains the veterinarian aspect presented by the first paper especially, and which fortunately is the important practical side of the question in the present day. All the four forms of the disease, acute glanders, chronic glanders, acute farcy, and chronic farcy, though differing considerably in external appearance, seem capable of originating in the same poison, and, we presume, are equally communicable to man from the same source. When once introduced into a human body, it is capable, like other morbid poisons, of multiplying itself indefinitely, so that discharges from the bodies of patients are poisonous to others. Instances are given where the disease arose from making the autopsy of a veterinary student who had died of glanders, from washing the clothes of a patient, and from dressing the wounds. It would be interesting to know how far the malady is modified for better or for worse by this secondary transmission of the virus.

Like glanders, "Hydrophobia" is interesting, not from its frequency, but from its fatality. They both present examples of an evil so picturesquely dire that the public is actually frightened into taking successful precautionary measures against them. The Gamgees say that in England since 1849 not a single case of hydrophobia has come under their observation, extensive as it is through their connection with veterinary colleges; while in France during the same period it has been very frequent. There was an awful outbreak at Lyons in 1864, and of the state of Paris in 1854 they paint a dreadful picture. We can scarcely fancy their meeting in the street "ladies unconsciously carrying their rabid pets to be visited by the professors!"

As to treatment, the writers speak with considerable confidence of the destruction of the virus by excision or cauterisation, before it has infected the system and is still localised in the part bitten. They rest their confidence mainly on the large experience of the late Mr. Youatt. But they carry it further, and tell us that "cases have undoubtedly occurred in which such a local treatment of a bitten part, even after the dreaded symptoms of recrudescence had set in, appeared to prevent the development of hydrophobia." They are therefore in favour of cutting out the scar or amputating the limb even at this stage.

It is considered as certain that hydrophobia is always dependent on a communicated specific poison, never idiopathic. The authors say that "all these theories" (of spontaneous origin in the dog) "are so absurd and so opposed to many of the facts which we must incidentally allude to in these pages, that we may state dogmatically they have no foundation in truth." As in the case of other contagious diseases, some individuals are mysteriously incapable of being affected. At the Lyons Veterinary College a pointer dog had been seventeen times experimentally subjected to the bites of rabid animals, and escaped hydrophobia. We should like to know whether it is possible that some may recover from mild attacks of the disease and so be protected. Is it possible that there is a milder virus, whose zymosis is a protection?

We congratulated syphilis for its escape from the grave of the Impetigines, and its arrival in its proper class. We confess we wish that accidentally, or, still better, intentionally, it had been ranged alongside of these animal poisons, whose history is known in their transmission from species to species. The only attempt at a physical account of its origin is that tradition which attributes it to the sores engendered by the newly-discovered natives of America, who introduced an irritant insect under the prepuce of their European lovers, either from malice or to stimulate their seared appetites. To Fracastori probably such a story sounded childish, but should it do so to us? Should not our knowledge of the benignant vaccinia, and the fatal equinia, pustula maligna, and hydrophobia, even if we have learned nothing from our experience of cattle plague, teach us not to despise the byways of history? The pathology of syphilis presents many points of resemblance to the poisons transmitted from animals to man—its communicability by inoculation only, its capricious infection, its long and uncertain incubation, its production of cutaneous local symptoms, its protective power against future infection, &c. It is possible to hope that, like some of them, it may be somewhat brought under the control of law and the plague stayed?

Where is "Pustula Maligna" which, in our opinion, should here have followed? Instead of it we have "Miliaria and Sudamina," which scarcely seems to belong here.

Then come the diseases of the whole system "determined by conditions existing *within* the body."

The first is "Scorbutus," by Dr. Thomas Buzzard, who, having been formerly attached to the army of Omar Pasha, has probably had more opportunities than most of seeing the disease on a large scale, and has had time to think about it since. In consequence he has produced a sound readable article, drawn from his own experience, which makes the reader better acquainted with scurvy and its treatment than many ponderous tomes. With matter foreign to this prime purpose it is very little diluted, full references and appropriate praise being given to other writers, for the use of those who would read more on the history. However, as he gives a brief notice of the abolition of scurvy from the British fleet, since he goes so far as to name Captain Cook, he should have included Sir Gilbert Blane, merely on the ground of *esprit de corps*.

From scurvy is rightly distinguished "Purpura," as being a disease of the blood-vessels rather than of the blood. It may, and very generally does, accompany scurvy; but, on the other hand, often occurs sporadically under quite different pathological conditions, on which the effect of remedies is very different. It is upon the ground of its being a disease of the blood-vessels that the effect of digitalis has been tried in it, a point which Dr. Hillier does not appear to see, though he recognises the true pathology.

The phenomena of "Rickets" are delineated by Dr. Aitken from the writings of physicians who have had experience and opportunities of observing this remarkable disease, namely, MM. Guérin and Guersent in France, and Copland and Jenner in this country, the latter of whom has given from his own observation the most detailed and original account of this disease that has ever been given by any physician.

The articles on "Gout," "Rheumatism," and "Rheumatoïd Arthritis" (or rheumatic gout), by Dr. Garrod, are an excellent summary of the observations on which he has been engaged now nearly twenty years, and which he has, from time to time, laid before the public in various forms. His view is well known of the pathology of gout, namely, that its essential feature consists in the retention of an excess of uric, and sometimes its congener oxalic acid in the blood. We are not aware that any serious objections have been found against it, though it has so long been before us. His explanation of the immediate cause of the localisation of the gouty paroxysms, namely, the defective vitality of the affected parts, is, according to himself, not so original; but yet we must take leave to credit him with bringing it into accord with modern pathology; and, moreover, we must thank him for his giving us the most striking evidence of the fact in familiar illustration. The great toe is farther from the centre of circulation than any other part of the body;

it has, in addition, much tissue imperfectly supplied with blood-vessels, and is also liable to frequent, if slight injury. Its vitality is, therefore, peculiarly defective, and hence it is the favoured seat of the first gouty paroxysm. And the other joints in succession follow very much in the order in which they abound in cartilage, tendon, and other non-vascular tissue. In men the ears are exposed to injuries and cold, hence they are often the seat of tophaceous deposits; in women they are covered up, and are rarely affected. An old injury will, as it were, attract gout to the damaged part, and cause it to linger there longer than in other localities.

The causes of the retention of uric acid seem to be powerful in a direct ratio with their power of impeding digestion. Over-eating, over-drinking of mixed liquors (for pure alcohol seems comparatively harmless), venereal excesses, prolonged study, mental depression, exposure to cold, all act in this way, and their generation of gout is notorious. The acknowledgment of another peculiarly lowering influence is, we believe, due to Dr. Garrod's own careful observation, namely, that of lead poison. Among his hospital patients affected with gout, as many as 30 per cent. have exhibited the characteristic blue line on the gums; many had suffered from colic, some from saturnine paralysis.

The view of the pathology of the gout, that it is a deficiency in the vital excretion of effete matter, exhibited in such persons and in such parts of these persons as have the lowest degree of life, explains why very opposite kinds of treatment, even when exclusively pursued by theorists of old, have with apparent justice been pronounced beneficial. We can quite understand the failure of expectancy or of genuine homœopathy, in comparison with either bleeding or brandy (to quote the extremes of Galen and John Brown). The first may act as an evacuant, the last may add strength to the failing innervation of the part affected. As might be expected, Dr. Garrod does not incline to either, but advises colchicum, bark, and iodide of potassium in acute gout; and in chronic, alkalies, with attention to the digestive functions, &c., &c.; in fact, the established orthopraxy of the present day.

In "Rheumatism" the same author exhibits a rare and valuable quality in an observer. Everybody knows that everybody has been trying, fairly and boldly, the proposal made by him ten years ago, to treat the disease with large doses of bicarbonate of potash, to the exclusion of other drugs. Very successful it was. At last some began to act the play of Hamlet without the Hamlet; that is to say, they tried the exclusion of other drugs without the bicarbonate of potash, and very successful *they* were, too.[1] In consequence, Dr. Garrod ranks his own pet child only on the same level with other persons' pet children—a philosophical act of virtue which we hope will be imitated. He is now trying the "quino-alkaline treatment;" that is, five grains of quinine decomposed to a carbonate, in a draught containing thirty grains of bicarbonate of potash, every four hours. The results are good.

Dr. Garrod must pardon us if we correct him on a small point of history. Sydenham was not, as is here stated, "a strong advocate" for bleeding in rheumatic fever. It is true that in the 1676 edition of the "Observationes Medicæ," he commends the bleeding cure; but four years later he tells us in his letter to the Warden of Keys (date 1680) that he had substituted a simple nutritious diet of whey for venesection with great success. "Neque me fefellit," says he, "diæta e sero lactis phlebotomiæ loco substituta." This he repeats in 1685, in the edition revised by himself.[2] Dr. Garrod will excuse our criticism the more readily, as we cite it for the sake of drawing a parallel between Sydenham's mind and his, and holding them both up for the imitation of physicians, as contrasted with the "cock-sure of everything" style of practice.

Curiously enough, Dr. Garrod does not seem to have tried these large doses of quina above mentioned in "Muscular Rheumatism." We have found a striking effect from the use of it in cases of lumbago.

The volume is concluded with a short practical paper on "Gonorrhœal rheumatism," by Mr. Brodhurst. We shall be glad of more from his pen.

The duties of an editor of all this matter are very responsible, and require dictatorial ability and administrative tact of no common order. He ought strictly to define the extent of each

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[1] The cases hitherto published in which no active drugs were given to a continuous series of unselected patients are the following—

	No. of patients.	Average No. of days in hospital.	
Under the care of Dr. Gull	37	27·2	'Guy's Hospital Reports,' 1865.
„ „ Dr. Rees	4		
„ „ Dr. Chambers	25	27·7	'Chambers' Clinical Lectures,' page 144, 3rd edit., 1864.

Two cases are also detailed by Dr. Russell, as treated at the Homœopathic Hospital in 1861, whose days in hospital were 23. ('The Treatment of Rheumatism,' &c., 1865, page 63.) A series of these observations would be valuable. Of 289 cases treated with drugs at St. George's the average time in hospital was 35 days. (Dr. Fuller on 'Rheumatism,' &c., page 71, edit. 1826.) We hope Dr. Garrod will try the experiment on a series, and not (as he proposes) on selected cases, which prove nothing.

[2] 'Th. Sydenhami Opera Omnia.' Sydenham Society's edition, page 299.

writer's subject, lest they overlap, and so give rise to useless repetitions, or perhaps contradictions. He must limit the space to be occupied by each, and see that the author does not, in attempting condensation against the grain, omit the general features of the landscape, and fill the picture with over-minute details of foreground. Above all, he should tyrannically erase a great deal, good in itself, and important to the writer from the trouble it has given him, but which does not fall in with the plan of the work; which might, in fact, be suitable for the encyclopædia, or other form of retrospect, but is out of place here. To say that Dr. Reynolds has performed this task perfectly, would be to attribute to him an archangelic power; and in the hope that, like all good players, he is willing to take hints from lookers-on at the game, who yet confess themselves unequal to taking his post, we beg to offer a few criticisms on which we hold to be his department.

Our first remark on opening the well-bound volume was, that we should have liked the edges cut. It saves much trouble, and keeps cleaner on the shelves. Having cut the leaves ourselves, we next remarked that nearly all the articles begin with a DEFINITION, which is an orderly and useful proceeding. It is like a marshaling by nature of the animals as they came out of the Ark. But, as in the book of Genesis, a "confusion of tongues" rapidly ensues, there comes a storm of heterogeneous learning, which some of the writers have headed SYNONYMES, and some SYNONYMS. For this we cannot discover the slighest use, nor paint to ourselves any idea of the sort of reader to whom it would be suited. Would a medical man about to practice in France or Spain, Sweden or Germany, instead of buying a dictionary, and learning the language, refer to the article on "Diphtheria," to see how the author believes it to be called in these countries? Would Mr. Marson's information of what "Smallpox" is named by Arabs and Irish Celts assist any researches in Nejed or Galway? In the bibliothecoïd variety of systematic writing these little Babels might find a place; here they are what Coleridge called an "ultra-crepidation." Each writer clearly feels himself in a ridiculous posture; like Claudio, in "Much Ado about Nothing," "he was wont to speak plain and to the purpose; and now he is turn'd orthographer; his words are a very fantastical banquet, just so many strange dishes." In consequence the task is slurred over carelessly, and, if at length, badly. Were any real orthographer or an ethnologist to take up the volume, he would be sadly amused. He might open it, as we did, at page 370, and read about diphtheria—

> "SYNONYMS.—Cynanche, Angina, Celsus; Synanche, Œtius Aurelianus; Ulcera Egyptica vel Syriaca, Aretæus; Crustosa et Pestifelentia Tonsillarum Ulcera, Ætius Amidenus; Ulcera Pestifera in Tousillis, Paulus Ægineta;" &c. &c.

Is it possible to perpetrate more blunders in four lines? Celsus, instead of making Cynanche and Angina synonymous, makes Angina the genus which he says the Greeks divided into three species—*συνάγχη*, *κυνάγχη*, and *καρασυνάγχη*. "Œtius Aurelianus" is an imaginary person altogether; probably Cælius Aurelianus is intended, who, however, is thus put out of chronological order, and who really adopts the same division as Celsus, with an unimportant addition; and in describing synanche, describes quinsey not diphtheria. If Aretæus had understood Latin (which is doubtful), he would never have written "Ulcera Egyptica," but Ægyptica. Moreover, it is not true that he uses the term, though he says other people did. Then why are Ætius and Paul of Ægina represented as speaking Latin? A few lines lower down we find Professor Rosen placed chronologically before Bard, and speaking both Latin and English; Louis begins with French and winds up in English; Bretonneau more naturally begins with English, and lapses through Latin into dubious French, "Diphtheritic Angina, Pellicular Angina, Diphtherite and Diphtherie, Bretonneau." In the rendering into Spanish an r is omitted and a t inserted; and even when thus corrected, "Garrotillo" means "quinsey," as well as "Diphtheria." One might just as well put in "Sore-throat" as a synonym. The Swedish also is wrongly spelt, as may be seen by a back reference to the author's article on Croup. In the next page the reference to Hippocrates is wrong; there are only fifty-four sections in the 5th book of the Epidemics. The case referred to is probably case xxxii of the 7th; the author, however, does not "give us the name of probably its first recorded victim," but that of her husband. Besides, it is difficult to believe that it really was a case of diphtheria, for the patient died of diarrhœa, which is rare in the advanced stage of that malady. The name which Hippocrates gives to the disease was used by him, according to Asclepiades, for catarrhal sore-throat ("humoris fluor, quem rheumatismum vocant," 'Aureliani de Ac. Morb.' iii, cap. 1) as well as for quinseys. Then we have Mr. Squire writing Ætius Amidenus in this, and Œtius of Amida in the succeeding page, meaning the same man; and Œtius Cletus for Ætius Cletus (the pseudonym of an Italian physician). But we must move on, or the reader will think we have stumbled on an unhappy specimen.

Passing over "Measles" and "Roseola," the authors of which kindly spare us the deep learning, we come to "Smallpox," and there find—

> "SYNONYMS.—Jadarí, Arabic; *Εὐλογία,* Modern Greek; Variola, Latin; Smallpox, English; the Pocks, Scotch; Galra breac, Irish; Petite Vérole, French; Blattern, German; Vaiuolo, Italian; Viruelas, Spanish."

Pause awhile for a few remarks on these words.

"Jadarí" expresses very badly the word used by Rhazes, الجلدي, which may be more approximatively written "Ul Juduree." But as these profess to be synonyms, why do we not have the real synonyms, "Nubukh" and "Humak?" In Greek the translator of 'Rhazes' into Greek translates the word just commented upon by *λοιμικὴ*, which ought certainly to have been inserted as the corresponding word. *Εὐλογία* means "a blessing," and is a technical term in ecclesiastical Greek for the benediction at the end of the service. The real Greek word for smallpox is *Εὐφλογία* or "fine eruption;" and it is certainly a vulgarism to confuse words of such opposite derivation—a vulgarism which is avoided by the Greek newspapers of the day. We pass to the next rendering. So long as people wrote Latin even as a learned language, as late as the last century, all those who had any pretension to scholarship wrote "Variolæ." It is the diminutive of "vari," a classical word for cutaneous eruptions, and in the singular would have been understood by a Roman under the Empire to mean "one little pimple." Variola may be good English or Scotch, but it certainly is not a Latin term. Take the next word—"the Pocks" is no more Scotch than it is English. Dorset uses it. When Syphilis is intended, an epithet is added in our best writers—"the great pox," or "the venereal pox," as Milton calls it in his 'Ready Way to Establish a Free Government.' In the next translation (for these should not be called synonyms) the Celtic "galra" simply means "disease," and is surely not needed in either Erse or Gaelic, "breac" being sufficient. The next is not absolutely wrong; but the reader ought to be told that modern German writers use the singular "Blatter" for the disease, and the plural "Blättern," or "Blattern," for the pustules (Schönlein 'Path. und Ther.,' iv, 259). Of the Italian, also, the more usual spelling, nowadays, is "Vajolo" ('Rendiconto dell' Ospitale Magg: in Milano,' 1862). In short, with the exception of the French and Spanish, there is not a single attempt at a translation which is not careless and useless, and therefore noxious.

In the next page Aaron, as in the authorised version, is changed into Ahron, though the same Jewish name is meant.

Turning to another part of the volume, we see "Rickets" a-top of the page, and eleven lines lower down "Ricketts" given as the English spelling, which it never was. We have it stated that Dr. Whistler called it "Morbus Puerili Anglorum, the Ricketts." Dr. Whistler was a great rascal, and on his death, in 1684, he was found to have purloined our college plate, but he was never capable of perpetrating two such solecisms as these. That, however, is not the worst of it, for we find his name placed chronologically before Glisson's, as a previous nomenclator, though the latter is rightly stated in Johnson's Dictionary, a sufficiently accessible book, surely, to have first described the disease. It is true, that Dr. Whistler's inaugural thesis was delivered in 1645, and the report of the committee on the subject, drawn up by Glisson, in 1650; but Glisson had made public his opinions many years before that, as is testified by a contemporary, Sir John Floyer ('History of Cold Bathing,' p. 77). This Whistler's dishonest appropriation of the property of his betters must not be allowed to be successful because two centuries have passed; nor should an Englishman's faith in his Johnson be lightly tampered with. Glisson must have restored to him whatever credit attaches to having transferred to science the popular name for this disease. In the next line Chuden is wrongly represented as using the barbarous word "Atrophium;" whereas he writes very fair Latin, always writes "Atrophia," and says it is a rendering of the vernacular "Das Abnehmen der Kinder." Then Dr. Darwin is set forth as calling it "Innutrio Ossium;" whereas he makes a separate species of "Innutritio Ossium," (for which Innutrio is a misprint), and of "Rachitis."

Next Dr. Young is unfairly dealt with; he was very particular about his etymology, especially in respect to the letter h; and so he calls the disease "Scrofula Rhachitis," which is perhaps the most correct method; whereas here he is especially selected as an authority for "Scrofula Rachitis." He is also unfairly dealt with as to his age, for Dr. Cumin, of Glasgow, who was alive only the other day, is placed before both him and Darwin. This puzzles any one who is attempting to identify the persons.

The French word "noruire," is a sad puzzle, which we are unable to unravel. A rickety child is said to be "noué," and the swelled joints are called "nœuds," but any substantive at all approaching what Dr. Aitken has quoted we are unable to find. And we confess that our experience does not lead us to have confidence in words we cannot otherwise collate.

Have we said enough to show the expediency of keeping clear of this pitfall of supererogatory erudition? The exhibition is (*experto crede*) very irritating to a commentator; he would not pick a hole in a man's coat, or any harmless necessary garment, but borrowed cheap jewelry incites him to apply the file. An ill-natured critic might prolong the operation; for we must beg the authors of the treatises quoted, to believe that we have fallen on them by haphazard, and not because they are sinners above all that dwell on this dangerous ground. And we may also allow them the comfort that poor Sir John Forbes, who did the bibliographic business for the 'Cyclopædia,' is equally open to criticism. We see no probable path to reform except total abstinence.

Perhaps a reader may say that these are trivialities—*aquila non capit muscas.*—True; but the flies are embalmed in amber, are lapped in the precious ointment of the apothecary, and they imperil the transparency of the one, and the sweet savour of the other.

Another fault we should wish to see corrected in a future edition is that of imperfect reference. Bare surnames are clubbed together between brackets as authorities, and not the slightest hint given as to the place or even the book referred to. Sometimes you do not know exactly what to make of it. When you are suddenly pulled up short in your reading by a " (Wunderlich)," is it a rough Teutonic exclamation or a proper name? Sometimes the very familiarity of the patronymic puzzles you. Suppose a student, who occasionally likes to get to the bottom of a thing, to be investigating the pathology of ague. At page 59, he learns that " when the fever intermits the urine then rapidly undergoes decomposition, and changes from acid to alkaline (Jones)." The name excites his interest; he hesitates about the fact; or he would fain know more. But among about 130 or so of that appellation practising in England, he finds from the Medical Directory, that some have published under comprehensive titles, such as 'Manuals of Pathology,' 'Cases and Observations,' &c.; another is a " contributor of numerous papers to *all* the Medical Journals, Trans. Roy. Soc.," &c. Even if he decides to exclude from his list all but metropolitan lecturers on medicine, he finds that one of these has written on intermittent nervous disorders, and another on renal diseases, and that both are contributors to periodicals. Where is his search to end? On this side of the Atlantic? or is it " Jones of Georgia," quoted at length a few pages further on, that is here also intended? In the Essay on Rickets there are about sixty specimens of such a bracketing of surnames without further reference, some of them being often repeated. Even the contribution of a Christian prenomen might be of value. In a dispute which arose in a certain mess-room as to the birth-place of Dr. Johnson, it was found, later in the evening, that high words might have been prevented, had it been known that one party referred to Dr. Samuel the lexicographer, the other to Dr. something else Johnson, surgeon to the —— militia. We think that for a work like the System of Medicine the rule should be, that unless a citation of authority is complete and accurate, it is to be omitted altogether.

Have we not shown Dr. Reynolds by this microscopy of the freckles on the skin of his offspring, that our feeling is one of the deepest interest in its intrinsic merits? We think he has given an earnest of accomplishing wisely and well a most difficult task, that of recording the present position of medical science in England. It is a task more difficult than has been undertaken by any of his predecessors, because this seems like the opening of a new era, and a great deal of what he will have to record bears promise for the future as often as fruit for the present, and we look to see his muse grown with the growing age. For that reason, however, the undertaking will be the most useful, and should be zealously supported by our progressive profession. More than content with this, we anticipate the forthcoming volumes—

> "As when a well-graced actor leaves the stage,
> The eyes of men are bent on him that enters next."

Review VI.

1. *Etudes Ophthalmologiques. Traité Théorique et Pratique des Maladies des Yeux.* Par L. Wecker, Docteur en Médicine des Facultés de Wurzbourg et de Paris, Professeur de Clinique Ophthalmologique, &c., &c. 8vo, tome premier, pp. 848; tome second, premier fascicule, pp. 412. *Paris:* Delahaye. 1863-4-5-6.

Ophthalmological Studies. A Theoretical and Practical Treatise on Diseases of the Eyes. By Dr. Wecker.

2. *A Treatise on the Principles and Practice of Ophthalmic Medicine and Surgery.* By T. Wharton Jones, F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, &c., &c. Third Edition, recast, much enlarged, and illustrated by numerous additional engravings. Small 8vo, pp. 806. *London:* J. Churchill and Sons. 1865.

3. *A Guide to the Practical Study of Diseases of the Eye; with an outline of their Medical and Operative Treatment.* By James Dixon, F.R.C.S., Surgeon to the Royal London

Ophthalmic Hospital, Moorfields. Third Edition. 8vo, pp. 370. *London:* John Churchill and Sons. 1866.

4. *A Handy Book of Ophthalmic Surgery, for the use of Students and Practitioners.* By J. Z. Lawrence, M.B., F.R.C.S., Surgeon, and Robert G. Moon, House Surgeon to the Surrey Ophthalmic Hospital. With numerous illustrations. 8vo, pp. 160. *London:* Hardwicke. 1866.

5. *Étude Ophthalmoscopique sur les altérations du Nerf Optique, et sur les Maladies Cérébrales dont elles dépendent.* Par Xavier Galezowski, Docteur en Médecine de la Faculté de Paris, &c., &c. Ouvrage orné d'une planche en chromolithographie, et de trois figures intercalées dans le texte. 8vo, pp. 179. *Paris:* Leclerc. 1866.

The Ophthalmoscopic Investigation of changes in the Optic Nerve, and of the Cerebral Maladies on which they depend. By Dr. Xavier Galezowski. With one plate in chromolithography, and three figures in the text.

6. *Lectures on Diseases of the Eye; referring principally to those affections requiring the aid of the Ophthalmoscope for their diagnosis.* By N. C. Macnamara, Surgeon to the Calcutta Ophthalmic Hospital, Professor of Ophthalmic Medicine and Surgery, Medical College, Calcutta. 8vo, pp. 265. *Calcutta:* Lepage, 1865.

7. *Die Physiologische Optik. Eine Darstellung der Gesetze des Auges, und der Sinnesthätigkeiten überhaupt.* Von Dr. Hermann Scheffler. In zwei Theilen, mit 521 in den Text eingedruckten Holzstichen. 8vo, pp. 1035. *Braunschweig.* 1864-5.

Physiological Optics. An Exposition of the Laws Governing the Eyes, and the Senses generally. In two parts, with 521 woodcuts among the text. By Dr. Hermann Scheffler.

8. *The Optical Defects of the Eye and their Consequences, Asthenopia and Strabismus.* By J. Z. Lawrence, M.B., F.R.C.S., &c., &c. 8vo, pp. 112. *London:* Hardwicke. 1865.

9. *On the Means employed for Correcting the Inverted Image on the Retina of the Eye.* By Joseph Swan. 8vo, pp. 31. *London:* Bradbury, Evans, and Co. 1865.

Of the various works mentioned at the heading of this article, all bearing in some degree upon ophthalmic science, the first and second stand alone in their claims to deal with ophthalmology as a whole. Dr. Wecker tells us in his preface that it has been his object to present "a succinct picture of the maladies that affect the organ of vision, and of the great progress made by this branch of medicine in recent times." Mr. Wharton Jones asserts that "in preparing this new edition of my work on the 'Principles and Practice of Ophthalmic Medicine and Surgery,' I have endeavoured to make it as complete an exposition as possible of the subject in its present advanced state." The only apparent difference between the two writers is, therefore, that Mr. Wharton Jones has aimed at being "complete" and Dr. Wecker at being "succinct;" and it is scarcely possible to avoid the institution of a comparison between their respective performances. The first idea which such a comparison suggests is that one, or both of them, must have failed in the special object of endeavour. The work of Dr. Wecker is a large octavo, very closely printed. It already extends to 1260 pages; and the concluding fasciculus, containing the whole subject of the disorders of the ocular muscles, of refraction, and of accommodation, has not yet appeared. The work of Mr. Wharton Jones is of the small size of Messrs. Churchill's well-known manuals, and contains 806 pages, a good deal broken up by the headings of sections and subdivisions, and still more by 173 woodcuts scattered among the text. It is obvious, therefore, that if Dr. Wecker be succinct, Mr. Wharton Jones must be very far from being complete; and that, if Mr. Wharton Jones be complete, Dr. Wecker might almost be called diffuse.

We hasten to say, however, that to this charge, in our judgment, Dr. Wecker is not liable. His work has been somewhat lengthened out by his having obtained the assistance of other writers, of special skill in certain departments of inquiry, to deal with subjects that they have made their own. Thus the anatomy and physiology of the conjunctiva are described by Professor Krause; the anatomy and physiology of the cornea, sclerotic, iris, and choroid, by Professor Manz; of the orbit, eyelids, and lacrymal canals, by Professor Henke; and of the retina and vitreous humour by Dr. C. Ritter. The theory and use of the ophthalmoscope are described by Dr. Heymann, of Dresden. When such contributions are deducted from the whole, there still remains a work that is a rare example of fairness, sound judgment, luminous writing, and conscientious labour. On every subject of which he treats, Dr. Wecker appears to have studied the merits of all varieties of practice; and not only to have read, but to have mastered, the whole literature of the question; and then to bring an intellect matured by reflection and enriched by experience, to the task of balancing different hypotheses, and of deciding between different methods of treatment. He gives to every writer, of whatever country, the merit that is his own; and, when he disapproves of any course that is recommended by another, he states the grounds of his disapproval with scientific candour and moderation. Many of the questions that he discusses do not, in the present state of knowledge, admit of entire unanimity; and it is not improbable that differences of race and climate may do much to modify the experience of ophthalmologists in different countries, and may require to be met by differences in medical and surgical treatment. Many English surgeons, we apprehend, will find points upon which their own practice will appear to them to be better than that which Dr. Wecker advocates; but upon these very points they will find that all sides of the question are fully and fairly stated. This is nowhere more apparent than in the chapter upon the comparative merits of the different operations for cataract. It is difficult to find, in a book that deals exhaustively with a large subject, any fitting matter for quotation, but we have selected for this purpose the observations of Dr. Wecker upon the extraction of cataract together with its capsule, both as a fair specimen of his merits as a writer and as a description of an operation that is little known or practised in England—

"Persons who have carefully observed the results furnished by the different methods of extracting senile cataract have not failed to perceive that the greater part of the unfavorable consequences have been due to the retention of portions of the crystalline within the eye, and to the changes which these portions, together with the crystalloid, have undergone. Richter, in 1773, advised, on this ground, the removal of the lens within its capsule, and he proceeded by making gentle pressure upon the eye, after the completion of the wound. He recommends elsewhere that the surgeon should desist from such endeavours, if he experienced a resistance that could only be overcome by strong pressure. In 1799 Beer revived this method, and directed that the extraction of the lens in its capsule should be effected by transfixing the whole with a cataract needle, so as to disturb and rupture its attachments, in such a manner that it would follow easily, and might even be withdrawn upon the point of the instrument. This proposal occasioned much discussion, and even those by whom it was adopted appear to have praised it sparingly.

"Some time elapsed before the question was again raised, in 1845, by Christiaen, who, in order to extract the lens within its capsule, advised that pressure should be made upon the eye, even before the completion of the section. 'If it should happen,' says this author, 'that the crystalline should remain in its place when the section is finished (which is unusual when my directions have been followed) I give the eye a period of repose; then, lifting the upper lid and depressing the lower, and using suitable precautions, I place the flat of the curette upon the upper part of the globe, and, by a few compressive movements, occasion the issue of the lens.'

"Some years later the same attempts were renewed by MM. Moyne, of Naples, and Sperino, of Turin. M. Sperino has been good enough, very lately, to make me the following communication: 'The extraction of cataract without division of the capsule, which I have now for a long period practised with success, differs from the common procedure only in the omission of the second step of the operation (laceration of the capsule). As I have written, in the memoir published in the 'Transactions of the Brussels Congress,' I exert, in very gentle impulses, a slight pressure with a Daviel's spoon upon the sclerotic, on the side opposite to the section, assisted also by two fingers, which gently compress the globe through the eyelid. The eye being thus carefully pressed upon in its anterior third, the crystalline turns little by little, and, if the patient does not contract the ocular muscles too strongly, it often escapes without the loss of a drop of vitreous humour. I have never seen a greater loss of vitreous than has occurred in certain cases when the capsule has been opened. In soft cataracts the capsule is always more or less thinned, and will often give way when the lens is passing through the pupil; but it generally escapes, at least for the most part, with the crystalline. I have never seen secondary cataract in any of my cases.'

"Extraction of the lens within its capsule has been combined with modified flap extraction by my good friend, Dr. Pagenstecher, of Wiesbaden; who, at first, to judge from the short description of his method given in the work of Dr. Zehender, restricted its use to a limited number of cases. Dr. Pagenstecher, after having made a large iridectomy, proceeds, by simple pressure, to effect the complete removal of the lens. Quite recently he has written to me that he has determined to attempt, in all cases of flap extraction, the removal of the lens without rupture of its capsule, and never to operate without the employment of chloroform.

"Before receiving this communication from Dr. Pagenstecher, I had myself called attention to this method, which I now employ generally against all senile cataracts with hard nuclei. I shall content myself by giving nere a short description of the procedure, and will make known the results yielded by it when my cases amount to a sufficient number.

"I commence by causing the patient to inhale ether, preferring this anæsthetic to chloroform, on account of the slight intensity and short duration, or even the entire absence of the period of excitement which follows its use by the American method; and also on account of the rarity of vomiting, either during or after the operation. Complete muscular relaxation appears to be the condition most favourable to success.

"I make, as the first step, a semicircular flap, that corresponds precisely with the inferior half of the cornea. The section being complete, I recommence, for a few moments, the administration of ether, keeping the eye gently closed by pressure with a ball of charpie, in the palm of the hand. Being assured that anæsthesia is complete, I proceed to excise a portion of iris of the width of two millimetres. Lastly, I cause the lens to escape by gentle pressure through the medium of the eyelid, such as would be used, in the ordinary operation, after the laceration of the capsule. I must confess that, together with the lens, there is generally an escape of a little vitreous humour; but I can affirm, with M. Sperino, that the amount is not equal to that which is often lost in cases of common extraction.

"Before attending to the position of the flap, or applying the compressive bandage (which should be somewhat tighter than usual), I administer more ether, to prevent the occurrence of dangerous or involuntary movements at the end of the operation or during the dressing.

"It is needless to say that a cure is obtained, after this procedure, more quickly, and with much less irritation of the eye, than after ordinary extraction; and lastly, that the pupil presents a clearness seldom seen after any of the methods generally employed."

Dr. Wecker states, in his preface, that the two volumes of his work represent two parts; the first of which is intended to contain the information that ought to be possessed by every surgeon, while the second is chiefly addressed to specialists. In the first volume, therefore, we find the diseases of the conjunctiva, of the sclerotic, of the cornea, the iris and choroid, the eyelids, the orbit, and the lacrymal passages. In the second we find the diseases of the crystalline, of the vitreous body, and of the retina, as well as of the ocular muscles, and the disorders of refraction and accommodation. The last portion of the second volume is not yet published; but in all the earlier portions the practitioner, whether general or special, will find every information that he can desire, arranged in the manner most convenient for reference. We cannot doubt that the 'Études Ophthalmologiques' will long hold the first place among all the systematic treatises upon the subject.

For the sake of our own country, it is with much regret that we find ourselves wholly unable to speak of Mr. Wharton Jones's manual in any similar terms. The earlier editions of his book we have noticed upon former occasions; and we purpose to speak of the present one only with refercnce to the points on which it differs, or ought to differ, from its predecessors. Judging it from this aspect, in relation to modern ophthalmology alone, it has no claim whatever to represent the present state of science. On every question it is meagre; on most very inaccurate; and on many it is beyond measure unfair.

The improvements made of late years in ophthalmic medicine and surgery have been, in large proportion, traceable, either directly or indirectly, to the invention of the ophthalmoscope; and an accurate knowledge of this instrument, and of the principles on which it rests, are essential to any real comprehension of the present state of ocular pathology and therapeutics. It is quite possible that a surgeon who has acquired the power of seeing the fundus oculi, without understanding why he sees it, may yet be so far able empirically to interpret the more common morbid appearances as to save himself from gross errors in practice. Such a degree of knowledge as this may be sufficient for many general practitioners, but does not entitle its possessor to assume the position of a teacher and writer on ophthalmic medicine and surgery. To such a degree, however, and to such only, Mr. Wharton Jones appears to us to be entitled to lay claim. On p. 39 he has published a carefully drawn diagram, of which he says, that it "illustrates the course of the rays in the erect image exploration from the patient's eye to that of the surgeon." We cannot here reproduce the diagram, so as to point out all its errors, and must be content to say that they are such as no one with the most elementary knowledge of optics could fail to perceive. Chief among them is the position of the virtual image, which should be behind the object; but which Mr. Jones has placed in front of it, in the position of the actual image, between the eye observed and the observer. One careful and deliberate blunder like this is as convincing as a hundred, and fully establishes that Mr. Jones has not taken the trouble to master this part of the subject on which he writes. In actual practice the apparent position of the virtual image varies, within certain limits, in accordance with the refraction of the patient's eye, and furnishes one of the bases of the ophthalmoscopic diagnosis of ametropia. The means of forming such a diagnosis is among the things that it is Mr. Wharton Jones's office to teach. How can he perform this office when he proclaims himself to be unacquainted with, or rather to be wholly in error concerning, a very material point?

Passing on from his account of the optics of the ophthalmoscope to that of the various healthy and morbid conditions that the instrument reveals, we find this subject dealt with in about a dozen small pages, in which a space equivalent to two pages is occupied by five trumpery woodcuts, printed in red ink. The section is so meagre as to be worthless to a student, and to be wholly beneath the notice of a reviewer. It contains a barren enumeration of several different conditions, none of which are described with sufficient minuteness or accuracy to be recognised from the description; and, meagre though it be, it is not free from error. Thus, Mr. W. Jones writes—

> "Some degree of excavation of the papilla optica may be met with in quite healthy eyes. But such excavation of the papilla *cannot be*

confounded with the deep excavation with overlapping edges and peculiar course of the trunks of the retinal vessels seen in glaucoma."

Mr. Wharton Jones does not appear to know that there is at least one form of physiological excavation that simulates glaucomatous excavation very closely; and he is silent, of course, with regard to the difference between the two on which the diagnosis chiefly rests, namely, the presence or absence of a ring of nerve tissue, however slender, between the sclerotic foramen and the edge of the pit. In cases of commencing glaucoma, in which alone the diagnosis is of any practical importance, a knowledge of this difference is of great value. Mr. Wharton Jones's description of actual glaucomatous excavation is contained in ten lines; and is only true of confirmed cases of the disease, almost or altogether beyond the reach of treatment.

In describing what is commonly known as "interstitial keratitis," but which Mr. Wharton Jones prefers to designate "acute parenchymatous corneitis," he has the fairness to state that "Mr. Jonathan Hutchinson has satisfactorily established the fact, that a hereditary syphilitic taint constitutes almost always the root of the disease." In spite of this admission, he proceeds to say that the treatment "is to be conducted very much on the same plan as the treatment of phlyctenular ophthalmia," and he does not even hint at the employment of iodide of potassium as a remedy.

From these comparatively small matters we pass on to see how Mr. Wharton Jones deals with some of the greatest recent achievements of ophthalmology. We will select three subjects —glaucoma, cataract, and ametropia.

In the case of glaucoma, Mr. Jones commences his labours by defining the word to mean—

> "A peculiar greenish opaque appearance, deep behind the pupil, changing its seat according to the direction in which the light is admitted, being always most concentrated on the side opposite the light. This appearance occurs in very different degrees, from a *greenish gray reflection, barely discernible,* to a *grass green opacity.*"

He afterwards describes "simple glaucoma" as follows—

> "Here we have the glaucomatous appearance behind the pupil, but the eye, *in other respects,* appears quite healthy; the cornea clear, the pupil lively, the consistence of the eyeball normal, and vision—with the exception that it may be presbyopic or myopic—good. Simple glaucoma is of frequent occurrence in old people. It continues for life, but does not necessarily become complicated either with cataract or amaurosis."

We may safely presume that everybody knows that the word glaucoma was originally derived from γλαυκὸς, and that it was used by Hippocrates and Galen much in the same sense that it is now used by Mr. Wharton Jones. Everybody knows, also, that the word has been used of late years to denote a perfectly well-marked group of morbid phenomena, dependent upon tension of the eyeball; while, on the other hand, the "simple glaucoma" of Mr. Wharton Jones is not a morbid condition at all. If we were called upon to define glaucoma, in the present acceptation of the word, we should leave its derivation and original meaning out of sight, and should say that it was a morbid condition of the eye produced by increased intra-ocular tension, attended by presbyopia, occasional obscuration of vision, coloured fringes around flames, contraction of the field of vision, anæsthesia of the cornea, excavation of the optic disk, and gradual loss of sight; with a liability to occasional inflammatory complications, acute or sub-acute, if the increase of tension were too rapid for the tolerance of the eyeball. This condition Mr. Wharton Jones can only recognise as a "species" of glaucoma; and he describes it, not in its incipient condition, or during its period of progress, but only as it is seen when complete. The attacks of inflammation that occur during its course he calls, for some inscrutable reason, "arthritic,"—*acute* or *chronic arthritic posterior internal ophthalmia.* His idea of the essential pathology of the disease may be gathered from the following passages:

> "Seeing that constriction of the small arteries of a part causes congestion of the capillaries and venous radicles to which they lead, and seeing that *the fundamental morbid condition of the eye in glaucoma is great venous congestion*, with constriction of the small arteries, we can understand how it is that belladonna or atropia applied to the eye in glaucoma aggravates the symptoms.
>
> "The venous congestion of the choroid and retina in glaucoma, as we have seen, *causes some degree of intra-ocular distension.*"

Having thus carefully and deliberately put the cart before the horse, Mr. Wharton Jones proceeds to speak in a rambling and contradictory way about iridectomy. He seems to have attempted this operation once or twice, in cases in which the proper time for it had been suffered to elapse; and he shrinks equally from recommending or from condemning it. Thus he says, "Iridectomy is *commonly said* to operate beneficially by removing intra-ocular distension, and thus relieving the retina and ciliary nerves from pressure. *No doubt it appears to produce some such effect,* but how does it operate in removing the intra-ocular distension? is the question." With great submission to Mr. Wharton Jones, we think the "question" is, or rather once was,—*Does* it remove the distension? or does it only "appear" to do so?—and that the "how" might be left for future consideration.

In another place Mr. Wharton Jones writes—

> "Although by iridectomy the inflammation was, *in the case of acute glaucoma under notice*, subdued, and the sight somewhat improved, there was not a restoration of what may be called *good sight.* In chronic glaucoma, the result of iridectomy is much less favourable. We have only to look through the table of cases in the Ophthalmic Hospital reports, to be satisfied of the inefficiency of the operation in chronic glaucoma.
>
> "*It is thus perceived how unfounded the claim is which has been set up for iridectomy as an unconditional cure for glaucoma.*"

It is hardly needful to point out that the only claim ever set up for iridectomy as a cure for glaucoma is limited by very strict conditions indeed, especially in respect to lapse of time; and even the imperfect account which Mr. Wharton Jones gives of the solitary "case" on which he founds his argument, shows that these conditions were not observed. We think it probable, too, that the operation was very inefficiently performed. Mr. Jones says, with reference to the performance of iridectomy, as a means of preserving sight when the premonitory symptoms of glaucoma are present—

> "That iridectomy and extraction of the lens combined may not permanently secure the eye against an attack of acute glaucoma, was shown by a case of cataract I treated in the hospital some years ago. The patient was a woman about sixty years of age, and the eyes were at the time otherwise quite healthy. I performed extraction on one eye, and in making the section of the cornea, the iris, in consequence of a premature escape of the aqueous humour, fell before the edge of the knife, and a very considerable piece of it was cut out. . . . . The woman enjoyed capital sight, and continued to do so for some two or three years. At the end of that time she one day presented herself at the Eye Infirmary, complaining that the sight of the eye was totally gone. . . . On examination I found the eye—the eye on which *iridectomy* and extraction had been performed —hopelessly glaucomatous."

Now among the conditions limiting the usefulness of iridectomy, none has been more stringently insisted upon than the necessity for removing the whole breadth of the piece of iris cut out, *up to the ciliary margin.* Every writer upon the subject has pointed out that unless this be done, no permanent benefit is to be expected. It cannot be done by an accidental cut from a cataract knife; and therefore Mr. Wharton Jones, with strict verbal accuracy, but extreme impropriety and un-fairness, applies the word *iridectomy* to a proceeding totally different from that which the word is usually employed to denote; and argues, from the necessary failure of the former, to the inutility of the latter. Either Mr. Jones knows the rules that have been laid down for the performance of iridectomy or he does not know them. In the former case, what is his honesty as an advocate? In the latter case, what is his fitness to be a teacher?

In another part of the volume Mr. Wharton Jones gives instructions for the performance of iridectomy. In these instructions he makes no mention of the necessity of removing the whole width of the piece of iris, and he directs the iris scissors to be held and used in a manner that would render such a complete excision impossible. He gives a caution against wounding the capsule of the lens with the points of the iris forceps; but no caution against wounding it with the knife. He gives a figure of a *straight* knife for the puncture, and although he says that the operation should generally be performed on the upper portion of the iris, he makes no mention of the bent knife necessary in order to puncture safely in this region. May we not infer that Mr. Wharton Jones has never seen iridectomy properly and effectually accomplished? and that he has never perused the writings of the distinguished surgeons by whom it was first practised and advocated?

In order to arrive at the whole of our author's teaching upon the subject it is necessary to turn away from the section upon glaucoma and the section upon iridectomy to that upon "arthritic posterior internal ophthalmia." Under this head, Mr. Wharton Jones says that the prognosis is very unfavorable, and supports his position by relating the cases of patients whom (if they consulted him of late years) he suffered to become blind without resorting to the operation that would probably have cured them. He advises a treatment by leeching, calomel, colchicum, and Dover's powder, "for several nights," and free action on the bowels. *If this treatment prove ineffectual, iridectomy is to be had recourse to.*

Upon this, therefore, one of the most important questions of modern ophthalmic surgery, Mr. Wharton Jones's knowledge appears to be limited to a vague idea that somebody thinks that cutting out a bit of the iris, any how, is an "unconditional" cure for glaucoma. He blows hot and cold in a feeble, uncertain way; has performed something intended to be the operation once or twice, when it was well nigh too late, with no great benefit, argues against its utility, and yet recommends its performance when other treatment has failed. Of precise or trustworthy information upon the various questions involved, his book contains absolutely none; and it does not even contain a *résumé* of the opinions of the surgeons by whom iridectomy has been advocated, or of those by whom it has been opposed. Of the alternative operations proposed by Mr. Hancock and Mr. Vose Solomon, there is positively no mention whatever.

We pass on to cataract, by common consent one of the most important diseases of the eye that can fall into the domain of surgery. Its great frequency, the blindness that it produces, and the perfection of the cure frequently obtained, all combine to invest it with extreme interest. It is well known to most of our readers that ophthalmic surgeons have been engaged of late years in persevering endeavours to improve cataract operations; in order to obtain a larger per-centage of successful cases than was yielded by the methods formerly in use. These methods, when successful, left nothing to be desired; but they were not successful so often as could be wished. First, Professor von Graefe, then Dr. Mooren, then Dr. Jacobson, then Dr. Schuft, then Mr. Critchett, Mr. Bowman, and others,[1] introduced one improvement after another; and, lastly, von Graefe has adopted an entirely new method, of which, after practising it in many cases, he speaks in the highest terms. The methods of Drs. Mooren and Jacobson, which consist essentially of iridectomy in combination with flap extraction, are not left wholly without notice by Mr. Wharton Jones. He devotes to them one entire page of his treatise, and he even incidentally mentions Dr. Mooren's name. In a vague sort of way, Mr. Jones says that there is no particular harm in iridectomy; that it even produces "less liability to the supervention of iritis; and perhaps, also, to the suppuration of the cornea;" and that it "may be had recourse to" in certain cases. Upon the brilliant results obtained by those who have systematically practised it in extraction cases, Mr. Jones is silent; and his single page leaves the idea that it is a harmless sort of proceeding, which a surgeon may practise without incurring blame, if he does not mind the trouble.

The methods of Drs. Mooren and Jacobson have, however, been cast into the shade by the plan of scoop operation advocated by Dr. Schuft, and which, modified as it has been by Critchett, Bowman, and von Graefe, bids fair to supersede, as in the hands of many surgeons it has altogether superseded, the semicircular flap operation. Mr. Wharton Jones devotes *nine lines* to a mention of Dr. Schuft's operation, and the improvements of the other surgeons above named he *does not mention at all.* It is positively true, that a book professing to teach the present state of ophthalmic surgery contains no reference, of any kind, to the methods of cataract extraction which, in the great hospitals at Moorfields and Berlin, have well nigh superseded all others.

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[1] See a notice which appeared in our last January number (p. 145) on papers by Messrs. Teale, Critchett, and Bowman, regarding cataract operations.

Concerning these improvements there is, in the minds of those who have practised them, some difference of opinion, arising from the alleged frequency of residual capsular opacities after the new operations. But as Mr. Jones does not mention the procedures themselves, he does not, of course, mention the objections to them; neither does he mention the method of extracting capsule and lens together, to which we have already referred in our notice of Dr. Wecker.

The question necessarily arises,—What does Mr. Jones say about cataract extraction? The answer is, that he devotes nine pages and a half, and seven woodcuts, to a description of the obsolete and unjustifiable operation of couching; and thirty-three pages, with nineteen woodcuts, to the operation of flap-extraction, which, as we have seen, is beginning to fall into disrepute, in consequence of the invention of methods that are found to be more uniformly successful. In the whole of this portion of the volume the progress of modern times is entirely ignored—as much with regard to diagnosis and after-treatment as with regard to the operations themselves; and the whole section might have been written fifteen or twenty years ago. We have not had the curiosity to examine the first edition, published in 1847; but we think it highly probable that the section on cataract operations remains almost unchanged.

The subject of ametropia, or of the disorders of refraction, is, of course, almost entirely new; and Mr. Wharton Jones states, that the "recently published researches of Dr. Donders are fully considered."

The "full consideration" appears to us to amount to this: Mr. Wharton Jones has read the great work of the Utrecht Professor, and has endeavoured to condense it into fifty-five pages; an achievement only to be performed by the omission of all that is really important, and of all the details that require to be known in order to the application of the principles in practice. The perusal of these fifty-five pages might possibly impart some very vague and cloudy notions with regard to the general nature of ametropic affections, but could not possibly instruct the practitioner how to prescribe spectacles for any individual patient. The whole section would have been better omitted.

Mr. Jones, however, has not been content with simple condensation, but has thought it necessary to be critical and original as well. Thus he disputes the very keystone of Professor Donders' teaching—the doctrine that the eye is passive when adjusted for its farthest point; and not only asserts that the question is "open," but expresses his belief—

"That the adjustment of the eye for vision at a distance depends on muscular contraction, as well as adjustment for the vision of near objects, and that the real state of relaxation is that in which there is an intermediate degree of adjustment."

He does not appear to see that, if this belief were well founded, all persons becoming presbyopic would experience dimness of vision for remote, as well as for very near objects, and would begin to require concave spectacles for far distances, at the same time that they began to require convex spectacles for reading.

We have neither space nor inclination to pursue this subject further, and it has not been our object to exhaust the long catalogue of Mr. Wharton Jones's shortcomings. For his own sake, as well as for that of the profession, we greatly regret that he did not place the revision of his manual in the hands of some surgeon really acquainted with modern teaching and practice, and sufficiently conversant with optics to understand the principles involved in the use of the ophthalmoscope and the selection of spectacles.

The new edition of Mr. Dixon's well-known 'Guide,' although it has no claim to completeness, contains descriptions, of admirable lucidity and conciseness, of many of the more frequent forms of eye disease; and enunciates rules and principles of treatment founded upon large experience and careful observation. In a former edition Mr. Dixon laid himself open to well-merited censure by writing somewhat at random about the ophthalmoscope; and by expressing, about iridectomy, strong opinions which daily experience was refuting. These blemishes could not fail to diminish the confidence of his readers in his general accuracy and trustworthiness as an observer and a practitioner; and believing, as we do, that they were accidental, we have the greater pleasure in saying that they are removed. On such subjects as the disorders of accommodation and refraction, and the theory and practice of ophthalmoscopy, Mr. Dixon now displays a judicious reserve—writing a few pages only, and informing his readers where to seek for the more detailed information that it is no part of his plan to supply. These few pages, moreover, although necessarily meagre, appear to be accurate; and, with the remarkable exception of a mis-statement about the right position of the axis of a cylindrical lens in astigmatism, contain nothing which the student who trusted to them would be afterwards called upon to unlearn. It would be too much to affirm that they are of any great practical value, or that their entire omission would have materially diminished the usefulness of the volume.

The third and fourth chapters are devoted to an account of the disorders of the conjunctiva and of the sub-conjunctival tissue, on which Mr. Dixon writes with his accustomed brevity and clearness. He mentions the inoculation of the discharge of purulent ophthalmia as a remedy for the corneal vascularity and opacity produced by granular lids, and counsels its employment in extreme cases; but he does not mention the operation of peritomy, or the excision of a peri-corneal zone of conjunctiva, which, both alone and together with inoculation, has been found useful by some of his colleagues.

The fifth chapter treats of diseases and injuries of the cornea; and is chiefly devoted to inflammation, ulcer, and the disease seen in the subjects of inherited syphilis. There is a judicious caution against the use of nitrate of silver in cases of keratitis; but the directions for the treatment of ulcers of the cornea are extremely defective, or, indeed, it may be said that scarcely any are given. There are, perhaps, no forms of disease in which there is more gain from well-considered local treatment; and such resources as the yellow oxide of mercury ointment, the use of dry calomel, Mr. Critchett's setons, and the compressive bandage, ought to be familiar to every practitioner. There has been too much tendency of late years (perhaps as a reaction from the nitrate of silver of Mr. Guthrie) to regard corneal ulcers as matters requiring chiefly constitutional remedies, because they mostly arise from constitutional causes. We admit the premises, but not the conclusion. The longer the ulcer exists, the more harm it may do to the eye; and it may generally be quickly cured by suitable local applications. It is true that it would perhaps reappear; but, if constitutional remedies be needed, they may be employed after the ulcer is healed just as well as before. We in no way advocate the neglect of constitutional treatment, or its premature disuse; but we strongly advocate the cure of the local disease by the plan that is most speedy in its operation.

Among other omissions in this chapter we find no notice of a condition that is not very uncommon—and that is very embarrassing to the inexperienced—the distension of the cornea that sometimes follows keratitis, the dropsy of the anterior chamber, or the pellucid staphyloma of various writers.

The sixth chapter gives a short account of inflammation and injury of the sclerotic.

The seventh chapter is devoted to the iris; and, in so far as it deals with iritis, is noteworthy for expressing Mr. Dixon's conviction of the great utility of mercury in the syphilitic forms of the disease. It is not less noteworthy for containing *no reference whatever to atropine,* except in one form of iritis, the "scrofulous"—a form about the very occurrence of which Mr.

Dixon spoke "hesitatingly" in the second edition, but of which he has since "observed several cases." In describing the treatment of such cases he says, "atropine is the only local application likely to be of service."

Now, during late years it has been taught by the leading ophthalmologists that the free use of atropine is the one thing that must never be neglected in iritis. Some cases may do well under one kind of general medication, some under another; some surgeons trust to mercury, some to opium. But all, almost without exception, use atropine, so as to paralyse the sphincter pupillæ, to prevent the formation of adhesions, or to break them through if formed. In his second edition Mr. Dixon amused the profession by an argument intended to show that atropine would not dilate the pupil in iritis; and that, if it did, the dilatation would be injurious. The argument was commonly regarded as a curious piece of perverted ingenuity; but it could hardly have failed to influence some surgeons to their own injury and that of their patients. The entire omission of any reference to the question, in this edition, is very remarkable, and, we think, is not praiseworthy. The remedy is far too powerful to be left unnoticed, and its use must be either very beneficial or very injurious. Would it not have been wise if Mr. Dixon had either recanted and confessed that he had been in error, or maintained and advocated his former opinion?

The eighth chapter, upon the "choroïd and retina;" the ninth, upon the "vitreous body;" and the tenth, upon the "lens and its capsule," contain nothing that requires especial notice.

The eleventh chapter is devoted to glaucoma; and, in the small compass of ten pages, contains a clear and well-written account of the various stages of the disease. In the early stage it is said that rest of the eyes, careful attention to the general health, and iodide of potassium internally, are to be recommended; but when the general question of treatment is opened, we read as follows—

> "At the beginning of the present chapter I have alluded to the obscurity that still hangs over the pathology of glaucoma. For the present we may be well satisfied with the knowledge that in iridectomy we have a means of controlling an otherwise incurable disease, and saving patients from the blindness which was formerly its inevitable result."

After describing the mode of performing the operation, Mr. Dixon proceeds—

> "The recovery of sight after a well-timed and well-performed iridectomy is most remarkable, in many cases almost amounting to the restoration of former good vision; but in other instances the

> patient is greatly annoyed by the dazzling appearance and the distortion of luminous bodies, the result of the over-large pupil, and the consequent flooding of the eye with light. Tinted glasses, an opaque diaphragm perforated by a narrow transverse slit—either with or without a low convex glass—or other optical contrivances, must be resorted to according to the circumstances of the case. But even if, with all these appliances, the patient is still annoyed by irregular refraction, this comparatively trifling inconvenience *is not to be put in comparison with the benefits of iridectomy, without which utter blindness must inevitably have closed the scene.*"

This reminds us a little of Saul among the prophets; and a reader unfamiliar with recent ophthalmic literature would not easily be brought to believe that Mr. Dixon had not only, in the second edition of his work, condemned iridectomy utterly; but that, within the last two or three years, he had attacked the operation and its advocates with such bitterness as to call forth a public remonstrance from a number of his own colleagues, and of other surgeons, who felt that both the matter and the manner of his strictures overstepped the bounds which custom has assigned to professional controversy. We rejoice in Mr. Dixon's conversion, but we should have rejoiced still more if he had referred candidly to his former opinions, and if he had paid some tribute to the sagacity of those whose practice he once assailed, and now complacently adopts. There is, however, one point on which Mr. Dixon is still at issue with iridectomists of far larger, because of far longer, experience than his own. He says—

> "Graefe and others lay particular stress upon the iris being removed quite at this point (its ciliary attachment), but I do not believe it to be at all essential. I have always performed the operation with a cataract knife. . . . The iris, consequently, is not divided quite up to its ciliary attachment; and yet I have found the result of the operation just as satisfactory as that first described."

Mr. Dixon's great difference with "Graefe and others" has not been sufficiently prosperous to make it very clear why he should insist upon this small one; which it is more than probable that time will remove. We believe the truth to be that an imperfect iridectomy, such as Mr. Dixon advocates, is of little or no use in chronic glaucoma, and that its good effects in acute or sub-acute cases, although often remarkable at the time, are generally wanting in permanence. Probably something would depend on the breadth of the portion excised; and perhaps Mr. Dixon may use his scissors very freely in this direction.

The remaining chapters are devoted in succession to "scrofulous and encephaloïd deposits" to the lachrymal apparatus, the eyelids, the orbit, and to the principal operations. Some remarks on chloroform, some appended notes, a set of Jaeger's test types, and an index, complete the volume. Of these latter portions we have little to say, and nothing but what is in their favour. The book possesses the unusual merit of being smaller than the editions that preceded it; and this fact alone, now that the general literature of ophthalmology is greatly increasing, must have compelled the author to omit much that must have occurred to him as proper to be set down. We cannot expect opposite kinds of merit; and a treatise that presents the student with a handy and intelligible account of the leading features of the more common ophthalmic diseases, within the space of less than four hundred pages, is not fairly open to censure on the ground of being incomplete. We are thankful to Mr. Dixon for what he has done; and, if we allude to what he has left undone, we do so in no spirit of unkindly criticism, but only in order that the student who finishes the volume may remember that its perusal will leave him with much still to learn.

The little work of Messrs. Laurence and Moon is extremely well calculated to give a sort of bird's-eye view of modern ophthalmology, and will be useful to students as a guide to observation and to more extended reading, and to practitioners who are desirous rather to know generally what is being done by ophthalmic surgeons than to work at eye diseases themselves. As far as it goes it is very clear and accurate; and it mentions nearly every subject of importance. Its fault is, if indeed that can be called a fault which is a necessary consequence of the small size of the book, that it is general in its statements, and omits many points of detail that are highly useful in practice. At the same time it is but fair to say that it scarcely contains an irrelevant or unnecessary sentence; and that it is an example of the kind of condensation that can only be accomplished by writers who are perfectly conversant with the subject of which they treat.

The work of Dr. Galezowski is a valuable contribution to the literature of a subject of considerable importanee, that is at present only in its infancy. He rests mainly upon the statement, supported both by clinical observation and by anatomical research, that the capillary circulation of the optic disk is a part of the vascular system of the brain, and that it is entirely independent of the central artery and vein of the retina. After referring briefly to the arterial supply of the brain, he proceeds—

> "Among these branches there are some especially destined to supply the central organs of vision, and worthy of particular study. They are the following:

"1. Some filiform branches that proceed from the pia mater to the chiasma.

"2. A considerable branch that proceeds from the middle cerebral artery to the optic tract on each side, and that may be called the *anterior optic artery.*

"3. Two considerable branches from the choroid plexus, that are accompanied by veins, and penetrate the optic tracts at the level of the posterior margins of the crura cerebri, forming a well-marked boundary line between the optic tracts and the corpora geniculata. They may be called the *middle optic*, or *geniculate* arteries.

"4. An arterial branch, entering at the posterior border of the testes. This may be called the artery of the testes, or the *posterior optic artery.*

"From these vessels, and from the twigs that pass from the pia mater to the optic tracts, there is formed an unbroken vascular network from the optic tracts to the papillæ; and to this network, independently of the central retinal artery proceeding from the ophthalmic, the roseate tint of the papillæ is commonly due."

After some further details, the author continues—

"We are now able to understand the manner in which cerebral diseases may extend to the papilla of the optic nerve.

"The direct communication that has been shown to exist between the capillaries of the papilla and those of the brain causes the atheromatous affections of the vessels of the optic tracts or chiasma to extend to the optic papilla, and to occasion atrophy, without the central vessels of the retina being attacked or even diminished in size. In such cases there will be simply a disappearance of the capillary vessels to which the optic disk is indebted for its rosy tint in its healthy condition.

"States of cerebral congestion may give rise to congestion of the papilla through the same direct communication.

"Affections of the spinal cord, of the cerebellum, or of the pons, may extend to the corpora quadrigemina, either by continuity of fibre or by contiguity, and may produce inflammation or atrophy of the central organs of vision, associated with amaurosis. . . . . During the last few years I have made repeated experiments upon rabbits, by dividing or producing inflammation of the chiasma and optic tracts. The former proceeding was followed by atrophy of the papilla, the latter by optic neuritis."

Passing on to the more practical part of the volume, we find Dr. Galezowski describing optic neuritis and optic peri-neuritis, the former usually the result of cerebral tumours situated near the optic tracts and chiasma; the latter of inflammatory affections of the membranes of the base of the brain. The atrophy of the papilla consequent upon these affections is said to be distinguishable by the indented or broken boundaries of the nerve. Other forms of atrophy are also described, as well as all the various changes in the nutrition and circulation of the disk that have been observed in connection with cerebral maladies.

It is quite certain, we think, that a careful study of ophthalmoscopic appearances cannot fail to disclose a good deal that may be of value to the profession in a manner at present unsuspected. Just as the laryngoscope has led to the discovery of a previously unsuspected internal aneurism, so has the ophthalmoscope led to the discovery of a valvular disease of the heart, or of changes in the kidneys; and it is quite possible that its utility in such directions may hereafter be very widely extended. The distinction insisted upon by Dr. Galezowski between the capillary circulation and the central vessels of the optic disk is one that will afford abundant material for future observers, and that can hardly fail to bear important fruit. At present, however, we are still in the state of expectation, and the treatise before us does but point out a track that may lead to valuable conclusions if rightly followed.

The lectures of Dr. Macnamara show that the labours of modern ophthalmology are studied and appreciated in India; and the descriptions and plates possess especial interest, as illustrating the different conditions of the ocular membranes in the dark races. The author has published under very great difficulties, the work having been set up by native printers unacquainted, or imperfectly acquainted, with English; and, in spite of revision, it contains a very large number of press errors, some of which affect the sense, and of misplaced stops. It contains also a few errors that may, perhaps, be due to the author; but to these we will not refer more particularly. In the first place, it is by no means certain who ought to bear the blame of them; and, in the second place, a new edition of the work, together with lectures upon other branches of ophthalmic surgery, will shortly be printed under more favourable circumstances. Dr. Macnamara displays great industry and research, a considerable knowledge of the literature of his subject, and a very decided capacity for original observation. We hail the appearance of his work with very great satisfaction; and feel that it cannot fail to be highly useful to Indian surgeons. The dark colour and great abundance of the choroïdal pigment, and the consequent opacity of the epithelium in the Hindoo, render the descriptions of the ophthalmoscopic appearances seen in Europeans in many respects inapplicable to him; and hence the descriptions and coloured drawings given in these lectures will occupy in the East the same place as those of Liebreich and others in more temperate climates.

The 'Physiological Optics' of Dr. Scheffler is a work of prodigious labour, evincing the possession by its author of a rare combination of physiological and mathematical knowledge. In this country we fear it will meet with few readers, since the same combination is necessary in order clearly to understand it. As a matter of pure science, such a treatise has long been a desideratum; and the article by Helmholtz in 'Karsten's Cyclopædia,' which has hitherto been almost the sole attempt to supply the want, has of late years been scarcely to be obtained, the publishers refusing to sell separately the part containing it. The section on spectacles will be the most interesting to medical readers, and will be found to contain some valuable suggestions, especially with regard to the use of decentred or prismatic glasses. In other respects the work is calculated rather for reference than for perusal, and it is by no means free from some of the traditional peculiarities of the German mind. We had marked some of the passages relating to spectacles for quotation; but find they would scarcely be intelligible without the diagrams with which the whole of the book is copiously illustrated. We must, therefore, be content to say that Dr. Scheffler, in this, and in all other departments of his large and complicated subject, displays great erudition, as well as great power of expressing his meaning in an intelligible form. On many questions his views are strikingly original and are enforced by very ingenious and well contrived experiments.

The subject of spectacles is dealt with by Mr. J. Z. Laurence in a small and readable volume, intended to make the ordinary rules for the selection of glasses, and the reasons for those rules, intelligible to any capacity. The 'Optical Defects of the Eye' furnishes a suitable companion to the surgical treatise by the same author, and possesses much the same general character. We think that no one, after carefully reading it, would be at a loss what spectacles to prescribe for any ordinary case of short-sight, aged-sight, or hypermetropia; but the section on astigmatism is not equally full and explanatory. This is less to be regretted, since it is seldom that the more complicated varieties of imperfect vision are brought under the notice of any but a special practitioner.

Mr. Swan has put forth a little pamphlet containing a view for which he claims originality, but which was advanced by Dr. Alison about thirty years ago. He thinks that the retina, in relation to the brain, is turned upside down by the semicircular curve of the optic tracts, and that hence the inverted retina image is corrected by the impression from its lower portions being carried to the upper portions of the central organ of vision. The lateral inversion is corrected by the decussation of fibres at the chiasma. A small amount of knowledge of the phenomena of vision would have prevented Mr. Swan from advancing a doctrine that is untenable and opposed to common observation. The mechanical arrangements of nature are seldom of so very elementary a kind; and men of science are now well agreed that the correction of the retinal image is due to the direction of the axes of the bacilli.

REVIEW VII.

Bidrag til Bedömmelsen af Födemidlernes Næringsværdi. Af Dr. med. P. L. PANUM, Professor i Physiologien ved Kjöbenhavns Universitet.—*Kjoebenhavn, F. Hegel,* 1866, 8vo, pp. 104.

A Contribution to the Estimation of the Nutritive Value of Foods. By P. L. PANUM, M.D., Professor of Physiology in the University of Copenhagen.

FEW subjects have of late years attracted more attention than the vitally important one of food, whether in its relations to the nutritive value of the several aliments or to the work which the various kinds of food enable the animal organism to perform. To the subject in its first aspect the distinguished Professor of Physiology in the University of Copenhagen contributes, in the above-quoted essay, the results of numerous and laborious original researches, to which he prefixes a clear and interesting review of the several opinions which have in modern times prevailed among investigators.

The well-known division of organic aliments into three classes, as proposed by Prout, has the advantage that it indicates groups which, at least in a *chemical* point of view, are very well marked. These classes are—1. *Albuminous matters,* consisting of, as essential elements, nitrogen, carbon, oxygen, and hydrogen. 2. *Fats or glycerides,* consisting only of carbon, oxygen, and hydrogen, and being capable of decomposition, by metallic oxides, into glycerin and fatty acids. The species of fat most important as constituents of our aliments are margarin and elain, which, in various proportions, constitute the main bulk of butter, swine-fat, goose-fat, and most vegetable oils. Tallow contains in addition stearin, and butter butyrin. 3. The so-called *carbohydrates,* as starch, sugar, gum, dextrin, and cellulose, containing oxygen and hydrogen in the proportion to form water, and hence sometimes considered as chemical compounds of water and carbon. The same cannot be said of the division into nitrogenous and non-nitrogenous aliments, a classification acceptable neither in a chemical nor in a physiological aspect, inasmuch as it not only throws together, as non-nitrogenous aliments, groups chemically totally distinct, as fats, carbo-hydrates, organic acids, alcohol, but also classes with the albuminous matters many nitrogenous substances which, although in small quantity, are used with food, notwithstanding that both their chemical composition and physiological importance are very different.

Formerly the albuminous matters and their immediate derivatives were looked upon as animal products, until in 1838 Mulder, and after him Liebig, showed that they were, from the first, always and exclusively formed in the vegetable kingdom. This discovery was followed by Liebig's division of aliments with respect to their physiological signification; the albuminous matters and their derivatives being looked upon as the proper or histogenetic or plastic aliments, while the fats and carbo-hydrates were considered to be the respiratory media. According to this view we must, in estimating the nutritive value of food, distinguish between its plastic and its respiratory qualities. But if it can be shown that fats and carbo-hydrates have the power of retarding the metamorphosis of tissue, it is evident that, in virtue of such influence, they will possess a histogenetic or plastic economical value. Other matters, too, have an indirect nutritive value, those, namely, which, by supplying the want of taste common to all the proper organic aliments with the exception of sugar, by their effect upon the digestive secretions, promote digestion, and so increase the actual nutritive value of the essential aliments. Such an "enjoyment value" belongs to those articles which supply an agreeable taste, smell, or appearance, to food in itself deficient in these qualities. But in a physiological point of view, and in a higher politico-economical sense, this value is of very subordinate importance. The respiratory value, too, indispensable as it is, must also be considered as subordinate. All organic foods have, in fact, a respiratory nutritive value, provided they are taken into the blood, but the daily waste of carbon is covered at an incomparably cheaper rate by fats and carbo-hydrates than by albuminates, of which latter so much more would be required for this purpose. The former, too, are produced much more abundantly in nature than the albuminates, and can with much less expense than these be procured in a tolerably pure and concentrated state. The albuminates are found in the vegetable kingdom usually mixed with such large proportions of wholly indigestible cellulose, and with other carbo-hydrates, that man can appropriate them from comparatively few vegetable products, in which they are met with in rather large quantity, particularly from the fruits of the Cerealia and the Leguminosæ, while the bulk of the albuminates produced in the vegetable kingdom must with great loss be collected with the aid of the organisms of the herbivora.

Hence it is the histogenetic or plastic nutritive value which is usually kept in view in investigations respecting the subject of the work before us, a value which is with apparent correctness inferred from the determination of the amount of albuminates or protein matters in the several aliments. The first method employed to this end was that of direct elementary analysis, the amount of albuminates being deduced from the proportion of nitrogen obtained from the food. But the latter may contain nitrogenous matters not at all allied to the albuminates, or it may contain them in an indigestible form, as in that of elastic or horny tissue, or of gelatinous tissue, to which, though digestible, Donné, Magendie, and others, attach but small nutritive value, as being incapable of replacing the albuminates. These considerations brought the method by elementary analysis into discredit. The inaccuracies of that by proximate analysis, and the fact that a food which might be very nourishing where there was great power of digestion might afford but little nutriment to a weak stomach, were equally fatal to this plan. Moreover, chemical analysis can afford no information as to the histogenetic economical value produced in the metamorphosis of the animal tissues by the influence which may be supposed to be exercised upon the latter by the non-nitrogenous aliments.

We are thus led to infer that the histogenetic nutritive value of food must be determined by a direct physiological investigation, for it depends upon the influence of the aliments upon the living organisms. Such an investigation ought to show comparatively the different influence of aliments upon the metamorphosis of matter of the essential animal tissues.

The final results only, however, of the metamorphosis of tissue, the changes of weight in the system, and the matters which, produced by the decomposition of the tissues, are separated with the excreta, are accessible to quantitative investigation. Of the matters excreted by starving animals, water, carbonic acid, and urea are, beyond all comparison, the most important. Of these, the last named is distinguished by the fact that it is an organic substance containing 46 per cent. of nitrogen, which can owe its origin chiefly only to the essentially nitrogenous tissues of the organism; hence it would follow that a quantitative estimation of the urea excreted by a starving animal may be considered as a tolerably accurate measure of the decomposition of the essential nitrogenous tissues during inanition. And this view we find borne out by the observations which have been made in patients suffering from typhus, pyæmia, and pneumonia, as to the proportion of urea on the one hand and the changes of weight and tissue on the other.

> "In these affections the patients excrete, when the disease has reached its acme, a much greater quantity of urea than they would excrete when supplied with the same amount of food in health. In typhus, according to J. Vogel, the excretion of urea rises to from 40 to 55 grammes; in pyæmia it may, according to A. Vogel, reach even to 80 grammes; in pneumonia it amounts to 50, 60, or even 70 grammes in the twenty-four hours. Precisely at the period when the secretion of urea in these diseases is greatest, the patients use only so small a quantity of food that it may be considered nothing, and the difference is therefore very striking when we bear in mind that an adult man in health on mixed diet usually secretes only a little more than 30 grammes (about one ounce) of urea, and without food about only half that quantity. At the same time we find that the patient's weight, flesh, and strength, while the diseases referred to are at their height, diminish in a much greater proportion than would be the case in healthy individuals if the latter were compelled to live on the same small amount of food as the patients alluded to. We are, therefore, certainly justified in inferring that these two phenomena stand in such a causal relation to one another that the large quantity of urea is formed from the large amount of nitrogenous tissue, especially muscular tissue, which is decomposed in the morbid process." (P. 21.)

The determination of the weight of the body alone cannot give any available measure of the nutritive value of the food, as one or two classes of constituents might be unduly increased at the expense of others; the quantitative determination of the carbonic acid excreted by the system under a definite diet might to a certain extent, and under certain limitations, afford an idea of the respiratory value of an aliment. Can a quantitative estimation of the urea supply us with a physiological measure of the histogenetic nutritive value of food? Urea can be derived essentially only from the nitrogenous tissues of the body, or from the albuminates of the food. But obvious difficulties present themselves in the way of determining how much of this principle may be formed from the one or the other source. The researches of Bischoff and Voit[1] upon the laws of the metamorphosis of matter in general have, however, done much towards removing, or at least towards lightening, these difficulties. But the space at our disposal will not admit of our following the author through his critical examination of these researches. We therefore pass at once to the second part of his work, containing an account of the important physiological investigations he has himself carried out in reference to the subject of his volume.

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[1] **'Der Harnstoff als Maas des Stoffwechsels' ('Urea as a Measure of the Metamorphosis of Matter'), Giessen, 1853. 'Die Gesetze der Ernährung des Fleischfressers' ('The Laws of the Nutrition of the Carnivora'), 1860.**

From the *résumé* of the history of his subject contained in his introductory chapter, on the leading points of which we have thought it well briefly in the foregoing pages to refresh the memory of our readers, Professor Panum remarks that it would appear to be necessary in the first place to ascertain, so far as possible, by a series of experiments with a pure albuminate, *the degree of accuracy with which it is possible, by quantitative determinations of urea, to discover how much albumen an individual can actually appropriate, digest and decompose, from an indefinite quantity of food, consisting solely of albuminous matter and water?* For these experiments the author, for various reasons, chose a dog in preference to other animals. On collecting the urine he ensured, at the close of each day, the emptying of the bladder by combining the use of the catheter with pressure on the abdomen and with the action of an exhausting syringe. The urine so procured was added to what had been voluntarily passed during the day in the zinc-lined box furnished with a glass under it—and so arranged as to catch all the fluid immediately on its being voided—in which the dog was confined.

The albuminate selected by the author was the gluten of wheaten meal, with which he was supplied to an unlimited amount by C. Nielsen, starch-manufacturer. This substance is probably the purest albuminate which can be produced in the fresh state. When quite freed from the starch it contains only gluten, water, and the small quantity of salt inseparable from all albuminous matters. It is then necessary only to determine the amount of water present, in order to ascertain how much dry and pure albuminate has been employed.

The author presents us with the results of his experiments in a tabular form. Table I refers to his investigations on feeding with gluten and water, with total absence of solid food. The daily notes exhibit the animal's weight at the beginning of the day in grammes; the alteration of weight; the amount of fresh gluten consumed, in grammes; the amount of water drunk, in cubic centimètres; the amount of dry gluten, in grammes; the total amount of water, in grammes; the amount of urine, in cubic centimètres; the amount of urea produced, in grammes; the amount of excrements, in grammes; the loss by perspiration; how many times the amount of fresh moist gluten exceeded the quantity of urea produced; the proportion between the urea produced and the dry gluten consumed.

Of course we cannot place before our readers all the details of this table; we must content ourselves with following the author in directing special attention to some of the more striking points.

The proportion between the amount of albuminate consumed and of the urea produced was sometimes very strikingly constant; thus, on May 30, 31, it was as 1 : 4·37; May 31, June 1, it was as 1 : 4·34; June 1, 2, it was as 1 : 4·35; the average being as 1 : 4·35. Again, on the 5th and 6th of June, when the same quantity of food was consumed, the proportion was almost the same—1 : 4·544.

"The smallest quantity of albuminous matter with which the feeding was continued for several days consecutively was 62·5 grammes of fresh gluten, containing 21·499—22·007 grammes of dry albuminous matter daily. This produced on the 27th, 28th of June 7·488; on the 28th, 29th, 7·810; and on the 29th, 30th, 7·517 grammes of urea. The proportion between the albuminous matter consumed and the urea produced is here as 1 : 2·92, or 1 : 2·77, or 1 : 2·86. If the first day be excluded, because, perhaps, the better feeding of the previous day, may still have had some influence, the average proportion will be 1 : 2·81.

"In four days, from the 6th to the 10th of June, the dog was fed with from 115·623 to 131·1 grammes of moist and fresh gluten, or from 39·296 to 46·112 grammes of dry gluten, producing from 11·682 to 13·248 grammes of urea. If the twenty-four hours from the 6th to the 7th be omitted, because the much more nutritious feeding of the previous day had evidently still some influence on the production of urea, there was consumed on an average 43·386 grammes of dry albuminous matter, producing on an average 12·117 grammes of urea. The proportion between the two is therefore found to be 1 : 3·58. The proportion of the production of urea to the consumption of dry albuminous matter is consequently, in full feeding, 1 : 4·35; in medium feeding, 1 : 3·58; and in slight feeding, 1 : 2·81. The proportional number which was here *experimentally* obtained in medium feeding, namely, 1 : 3·58, is, therefore, precisely the same as we should have got by *mean calculation* of the relation between full and slight feeding, for—

$$1 : \frac{4·35 + 2·81}{2} = : 3·58.$$

"The agreement between the observation and the calculation is, therefore, complete.

"The reason why the proportion between the consumption of albuminous matter and the production of urea appears to be different in feeding with large and with small quantities is evident, as the dog produces urea also in a completely fasting state, namely, as the table shows, on the second day of inanition, 4·746; on the third day, 4·483; on the fourth day, 4·019; on the fifth and sixth days, 3·822—3·886 grammes." (P. 49.)

In case of any difficulty in getting the dog to consume the desired quantity of gluten, Professor Panum found that touching the mass with a drop or two of milk, or applying it to a piece of meat, was sufficient to induce the animal—evidently in consequence of his acute sense of smell—to use it. He found it better, also, to present the gluten made into pellets than in one mass.

Table II exhibits the increase in the production of urea, corresponding to an increase in the amount of dry digestible and actually digested albuminous matter (gluten) contained in food consisting of pure gluten and water. The table runs by half grammes from 4 up to 30 grammes of urea. In the lowest part of the scale the increase of one gramme of urea corresponds to an increase in the consumption of albuminous matter of 4·890 grammes; in the top of the scale the proportion is 1 : 5·7606. The author found that he was able, with an almost unexpected degree of accuracy, from the amount of urea produced, to determine the quantity of albuminous matter the animal had consumed, provided that this quantity was for a couple of days consecutively equally great, that the whole quantity was consumed at the commencement of the twenty-four hours, and, finally, that no other matters had been present which could possibly complicate the result.

Table III exhibits the result of feeding with flesh (carefully prepared lean beef) and blood, the latter being a mixture of whipped ox and calf's blood, from which the fibrin was separated, and the amount of albumen in which had been ascertained. In this series of experiments the quantities of urea produced by continued feeding with the same quantities of meat were less constant, owing, doubtless, to the presence in the meat of many different matters. But it would appear, also, from this table, that the amount of urea produced was influenced in no slight degree by the quantity of water the animal had got, while no such influence could be traced in experimenting on feeding with gluten alone.

The quantities of urea obtained in this third series of experiments were so much more in proportion to the amount of dry albuminous matters contained in the flesh and blood than might have been expected from the results of the former experiments with gluten alone, that the author is led to conclude that the dry albuminous matters in the flesh and blood are more valuable than dry gluten in the proportion of 3 to 2. This difference may, however, be more apparent than real, and may be due in great measure to the fact that the most carefully prepared meat is not pure albuminous matter and water. The extractive matters, including creatin, creatinin, inosinic acid, &c., to which the peculiar smell and taste of meat is due, while they cannot form albuminous matters or animal tissue, are capable, nevertheless, as Frerichs and Wöhler have shown, of increasing the production of urea.

It will be possible, by continued experiments, to discover the true explanation of this interesting fact. For this purpose a consecutive series of experiments with meat and blood prepared in different ways would be necessary. In smoking meat all the extractive matters remain in it, but the albuminous matters (and perhaps also the extractive matters) undergo a chemical change, which will probably influence the solubility of the fibres in the digestive fluids. In salting, the extractive matters are in great part lost and pass into the brine; from this they may be again collected by removing the salt by diffusion through a bladder. In boiling, more or less of the extractive matters pass into the water, forming broth; by immersing meat in water already at the boiling-point the meat remains juicy and retains the greater part of the extractive matters, while the broth or soup is poor; in roasting, most of the extractive matters remain in the meat, if the roasting be not carried too far.

The author next directs his attention to the influence which the non-nitrogenous aliments have on the production of urea, and on its use as a measure of the histogenetic nutritious value of the food mixed with them.

As has already been mentioned, Bischoff and Voit had shown that carbo-hydrates and fat diminish the production of urea, both in an animal compelled to live without food and also when these aliments are used along with albuminous matters. The question is, whether this diminution of the production of urea is to be explained, as Bischoff and Voit think, by an increase of the other nitrogenous excreta in proportion to the production of urea, that is, by an altered mode of decomposition, or, as appears to the author to be fully as probable, by the influence of the decomposition of the non-nitrogenous aliments in lessening the metamorphosis of tissue, giving to these non-nitrogenous bodies an histogenetic economical value. But it is evident that the answer to this question will not influence the practical inquiry—*Whether the production of urea can serve also as a measure of the histogenetic nutritive value of such foods as along with albuminates contain carbo-hydrates or fat in any considerable quantity?*

Table IV exhibits the results of a series of experiments upon the influence of non-nitrogenous organic aliments (in this case starch and butter) on the amount of urea produced. The quantity of urea excreted is, in one column, stated in grammes; the next column exhibits the amount of urea which would have been obtained without the non-nitrogenous aliments (calculated according to Table II), while a third column shows the difference between the two. On the first day the production of urea fell off by 2·114 grammes from the calculated amount (7·641 gr.), the same result as would have been obtained if the dry gluten given with the starch and butter had been lessened; without them, from 21·499 grammes to 11·162, a difference of 10·337 grammes. By continuing the same diet on the next day the amount of urea was diminished by 3·591 grammes, a result which might have been obtained without the aid of the carbohydrates by diminishing the dry gluten given from 21·522 grammes to 3·96, making a difference of 17·562 grammes. The author would have wished to try the effect of starch without butter, but he found it impossible to get the dog to take the starch in this way. On the third day, however, he diminished the quantity of butter from 100 grammes to 33, while the amount of starch was maintained at 100 grammes. The results of his experiments with these proportions showed that 100 grammes of starch with 33 of butter diminished the production of urea in quantities proportionate, on the several days, to 10·659, 11·711, 12·811, 11·134, 8·456 grammes of dry albuminous matter (calculated as gluten), which would otherwise have been resolved into urea. Hence it may, with great probability, be assumed that the metamorphosis of the animal's own nitrogenous tissues was diminished on three days, when the dog got nothing but 100 grammes of starch with 33 of butter and a variable quantity of water, by 11·711, 12·811, 11·134 grammes of dry tissue, calculated as gluten. On the whole, it would appear from this fourth table that 100 grammes of starch with 33 of butter, in this dog, diminished the production of urea tolerably equally by about 2·2 grammes, corresponding to about 11 grammes of dry albuminous matter. But to decide the question whether this difference is owing to an altered (according to Bischoff and Voit), or to a diminished (as the author thinks probable) decomposition of albuminates of the tissues or of the food, fresh investigations would be necessary as to the other nitrogenous excreta, as well as to the question whether the nitrogenous tissues disappear as extensively on exclusive feeding with carbo-hydrates and fat as with total want of food, or whether their loss of weight is less on non-nitrogenous food than in complete inanition.

Table V exhibits the results of an inquiry into the histogenetic value of different kinds of bread and groats. In this investigation of the breads Nielsen's strengthening bread (Kraftbröd), prepared with additional gluten, takes the first rank; French roll, baked at the author's request with blood instead of water, comes second; the less white and less well-looking French roll from an inferior baker takes the third place; while the very nice-looking, white, and well-tasting French roll from the Court baker appears to have the least histogenetic value. The latter circumstance is probably due to the fact that the meal derived from the centre of the wheat, which is remarkable for the beautifully white appearance of the bread made from it, is much poorer in gluten than that procured from the outer layer. The author expresses surprise that the histogenetic value of the bread baked with blood was scarcely greater than that of the French bread from an inferior baker, a fact which he thinks is probably to be attributed chiefly to the albuminous matters of the blood losing, at the high temperature employed in baking, a portion of their original nutritive power. We cannot think that this is practically a matter to be regretted, as, notwithstanding the author's statement that "in the fresh state it is really well-tasting, and was highly approved of by children and other persons to whom I gave it," there would certainly, in our opinion, be something repulsive in the idea of using such bread.

Professor Panum next instituted a series of experiments to test the nutritive value of a new substance introduced into commerce from the starch manufactory of C. Nielsen, under the name of household or gluten groats. This consisted of flour kneaded with the gluten of an equal quantity of flour dried and ground into groats. In this case, too, it was found that the histogenetic value of the dough was essentially lessened by the heat employed in the drying of the mass.

In the case of French gluten meal, too, used for bread for diabetic patients, the histogenetic value calculated from the amount of urea produced was less than would, *à priori*, have been expected. Here, too, the drying of the meal seems to have been the cause of the difference, the albuminous matters having been rendered thereby less capable of solution in the digestive fluids.

Having made some remarks upon the changes of weight, the amount of the fæces, the loss by perspiration, and the state of the general health of the animal experimented on, the author appends his sixth table, exhibiting at one view the histogenetic value attributable, from the present investigation, to the foods in question. In a note he alludes to a prevalent opinion that the common dark rye-bread must be particularly nourishing, as it contains the meal next the husk, which abounds in gluten. He shows, however, that this is not the case, but that the dark rye-bread is undeniably the worst of all kinds of bread.

Professor Panum adds a chapter upon the political and industrial importance of the subject he has been considering, but the space allotted to us having been already exhausted, we cannot enter upon this portion of his work. In concluding, we must express our admiration of the accuracy of his investigations, and of their adequacy to their object, regretting that in a notice like the present we cannot lay their full details before our readers. Professor Panum's volume is an important contribution to our knowledge of the vital points it seeks to elucidate, and is well calculated to maintain the high character of its learned author as an original investigator and man of science.

REVIEW VIII.

1. *The Elements of Prognosis in Consumption, with Indications for the Prevention and Treatment.* By JAMES EDWARD POLLOCK, M.D.—*London,* Longmans, Green, and Co., 1865, pp. 423.
2. *On the Temperature of the Body as a means of Diagnosis in Phthisis and Tuberculosis.* By SYDNEY RINGER, M.D. *London,* Walton and Maberley, 1865, pp. 92.

THE works before us are valuable contributions to the literature of pulmonary consumption, a disease which, from its frequency, fatality, and hereditary character, will always claim the gravest attention of British practitioners.

The *diagnosis* of consumption, already remarkable from the extent to which physical signs contribute to it, has of late been rendered more perfect by the observation of an additional most objective sign—the temperature of the blood.

The *prognosis* of consumption, from the variety of conditions which precede, accompany, or follow the same structural lesions, is obviously most varied, and offers much that will well repay careful consideration and study. Hitherto this part of the field has been comparatively neglected, but in the second work before us it is fully recognised. A few words upon each topic in connection with these two works.

After explaining the particulars to be attended to in making observations upon temperature, Dr. Ringer advances the results of his inquiries in a series of "conclusions," in support of which twenty-four cases of phthisis are detailed in the second part of the work. The author states, however, in the preface, that his propositions are based on a far more extensive series of observations, made both by others and himself. The first of these propositions is—

> "1. There is probably a combined elevation of the temperature of the body in all cases in which a deposition of tubercle is taking place in any of its organs.
>
> "Thus, of the twenty-four cases here given, in twenty-one there was a continued elevation of the temperature of the body, and in these twenty-one cases the deposition of tubercle was proved during life by an increase of the physical signs, or after death by the post-mortem appearances."

This proposition naturally suggests to us the question, "What is it in phthisis which *causes* the elevation of temperature?—and to this Dr. Ringer replies in conclusion the second:

> "2. This elevation of the temperature is probably due either to the general condition of the body (tuberculosis) or to the deposition of tubercle in its various organs (tuberculization)."

Having observed that seven of his cases during the chief part of their course had no detectable disease excepting tubercle in different organs, the author believes that the cause of elevated temperature is to be found in one of the two conditions above mentioned. The more difficult question which remains to be decided is this—Is elevated temperature the sign of deposition of tubercle as a process, or of the state of system coincident with fresh tubercle already deposited? We say *fresh* tubercle, because two cases are given (cases vii and viii), from which it would appear that with *obsolescent* tubercle (cavities with thick, tough, fibrous walls, and shrunken, hard, gray granulations, enclosing cretaceous matter) no *continuous* elevation of temperature is observed. And this is a point of much interest in prognosis, as it would appear that there is a period in the history of tubercle at which a return to normal temperature may be regarded as a sign of abatement in the activity of the disease. Dr. Ringer, in his third conclusion, thus decides the question as to the cause of the increased temperature in phthisis.

> "This elevation of the temperature is probably due to the general condition (tuberculosis) rather than to the deposition of the tubercle (tuberculization)."

The arguments advanced to support this proposition are that the thermometer frequently shows a much elevated temperature prior to the development of sufficiently marked physical signs of local disease; that cases are common in which a very small deposition of tubercle corresponds to a very considerable and long-continued elevation of the temperature; and lastly, analogy with those febrile diseases in which the local lesion is unimportant is favorable to such a conclusion.

> "4. The temperature may be taken as a measure of the amount of

the tuberculosis and tuberculization, and any fluctuations in the temperature indicate corresponding fluctuations in the severity of the disease."

In the preceding proposition the theory was advanced that temperature is the mark of tuberculosis, or that state of system which accompanies tubercular deposit. In a case of phthisis two sets of phenomena are always present—a local and a general disease. Is the general disease, marked by depressed nutrition, quickened circulation, waste of tissue, &c., always proportionate to the local? We believe not. Whilst agreeing with the author that the thermometer may be an accurate measure of the tuberculosis, we cannot accept it as a guide to the amount of tuberculization. He has himself pointed out that a high temperature may exist even before auscultation and percussion can detect tubercular deposit, and that with a very small deposit we may have a high temperature. And this quite corresponds with what we observe in practice. Do we not frequently meet cases in which a small amount of local mischief is accompanied with much fever and constitutional disturbance, and *vice versâ ?* We may be wrong, but until further evidence is before us we would prefer the stethoscope to the thermometer in aiding us to determine the extent of tuberculization. We must therefore give a partial assent to the foregoing as well as to the following conclusions:

"5. The temperature is a more accurate indication of the amount of tuberculosis and tuberculization than either the physical signs or the symptoms."

Contrasting the value of thermometric observation with that of the symptoms in phthisis, especially with the indications afforded by weight, night sweats, and the pulse, the author attempts to confirm his proposition, that, whether taken individually or collectively, such symptoms fall far short in giving the information which the observation of the temperature alone will furnish. Granting that the author has succeeded in showing that, taken alone, the thermometric indication is preferable to any single symptom, we should still feel most unwilling to assign to the thermometer any other place than that of a valuable *auxiliary* to other means of investigation—those obtainable from physical signs, and a careful consideration of the antecedents and symptoms of the case. A prognosis founded solely on the degree of elevation of temperature would be as unreliable as one in which physical signs were exclusively regarded to the neglect of symptoms and general condition. With these reservations we willingly acknowledge our obligations to Dr. Ringer for bringing prominently forward the application of another "aid to diagnosis," and commend his interesting memoir as most worthy of the favorable attention of the profession.

In the preface to his work Dr. Pollock states that the observations it contains are the result of a minute study of cases at the Hospital for Consumption, extending over ten years. In the work itself, at page 57, the author more fully defines his aim. He remarks—

> "If, indeed, we could obtain information as to the kind of consumption which prevailed in each case or in each class of cases, and the average duration of such classes could be ascertained with some degree of accuracy, a foundation of practical knowledge of a most valuable description would thus be supplied. But to such questions as 'How long may we expect a chronic cavity in the lung to last?' or, 'What constitute the elements of chemistry in any given form of the disease?' there has been no reply in either the reports of registrars, the theoretical treatises of Germany, the more practical works of the French school, or in the matter-of-fact handbooks of practice which abound in our own country.
>
> "Such information is often found stored up in the personal skill of a single practitioner, and must be the result, not only of his labour over a multitude of cases, but of his individual tact, observation, and ability in the recognition and discrimination of the facts and circumstances which bear on his patient. But, although we all know such individuals, and refer our doubtful cases to their wisdom, there has as yet been no attempt to help the profession, as a whole, to the accurate discrimination of the varieties of phthisis and of the causes which affect the duration of the different forms of the disease to be met with in practice.
>
> "The attainment of such evidence as could place this kind of knowledge on a true basis is difficult, and some of the obstacles in the way have been already noticed. These have reference to the method of obtaining information, and are common to all investigations where large numbers of facts obtained by different observers have to be collected.
>
> "A higher and more difficult task has been attempted in this work, namely, such a natural classification of the disease as may lead to an easy recognition of its different forms, and such a description of the symptoms and history of the varieties found in actual practice as may furnish data of the more certain kind for estimating and calculating the duration of individual cases."

In pursuing this attempt, the first point which the author endeavours to determine is the actual average duration of phthisis, and in the outset he is met by difficulties which a determined perseverance and patient study of facts, laboriously elicited, alone can overcome. No class of cases described under a single name differ more widely than those of pulmonary phthisis;

and whilst there is perfect truth in the observation of Portal, "that the disease lasts from eleven days to forty years," it is clear that some more definite knowledge as to natural duration is requisite as a foundation for further calculations. The difficulties which oppose such an investigation depend upon causes on which we need not here enter, but the following extract from Dr. Pollock's work is sufficient to prove the careful and painstaking method in which his researches have been conducted:

> "The method adopted has been to note with care every single case in the presence of the patient, and this has been done personally by the author to the amount of upwards of twelve thousand. The state on leaving off treatment has been recorded, and the earliest symptoms have been sought for with scrupulous care. The physical condition has been invariably recorded at the time, and the complications and prominent symptoms noted as they arose. Out of this large number of cases, occurring among the out-patients of the hospital during a period of seven years, all the instances of tubercular disease of the lung of undoubted character have been selected, and such only as presented the physical signs of consumption retained. All spurious or doubtful cases have been rejected, and every affection but phthisis sifted out. About three thousand five hundred thus selected for analysis were thrown into groups, according to their peculiar features, every case-paper being separately studied, margined, and classified, and the duration at the period of leaving off attendance at the hospital recorded." (P. 65.)

In arranging the classes into which cases naturally group themselves it appeared necessary to assume a certain standard, to be called "ordinary phthisis;" it was decided that this name should have reference to *duration,* and include all cases which presented the ordinary features of phthisis and had lasted eighteen months. The other varieties are described as "acute phthisis," "chronic first stage," "chronic second stage," "chronic third stage," "diffused tubercle," "tolerated cavity," "strumous phthisis," &c. &c. Viewing the disease as a whole, Dr. Pollock believes that consumption lasts much longer than has been stated by pathologists or than is supposed by the public. He holds that modern methods of treatment—the supporting diet, cod-liver oil, and avoidance of all lowering agencies—have had much influence in extending the average duration of consumptive cases. But "average duration" will not help us in estimating the prognosis of a single case, and it is at this point we feel the value of Dr. Pollock's observations upon the varieties and subvarieties of the disease, and on the effect of external or accidental conditions.

With a very interesting chapter on the premonitory stage of ordinary phthisis, the author enters upon the detailed consideration of his subject. We do not recollect having anywhere seen a more graphic account of the diagnostic distinctions of phthisis and anæmia, diseases which in the premonitory stage of the former are not infrequently confounded. The two conditions are thus contrasted:

> "Reasoning theoretically, we may say that anæmia consists in a deficiency of certain necessary ingredients in the blood, while phthisis may have a similar deficiency, but has also a superadded contamination. Comparing symptoms, anæmia wastes those organs and tissues dependent on an abundant supply of red blood; phthisis wastes all tissues by furnishing them with impure blood, and leaves a residual deposit. The phenomena of anæmia are those of insufficiency; those of phthisis, of impurity of the vitalising fluid.
>
> "The results of both classes of disease are more strikingly divergent than their symptoms. Few disorders of blood origin are so manageable as anæmia. Fresh air, light nutritious diet, exercise to promote blood changes, and the metallic tonics, with warm aperients, do wonders for these poor faded plants, which so remind us of London gardening! In phthisis we have to eliminate a poison as well as to restore lost healthy constituents of the blood; hence the uncertain issue of our cases, the slight contamination of some terminating in minute deposit tolerated for years, the more active constitutional disorder in others rapidly fatal; or the deposit softened, excavated, and the patient righted for the time. These and other results of tubercle, which are to be again treated of, illustrate the nature of the early disorder; but for purposes of prognosis and diagnosis sufficient has been said. Anæmia and phthisis are rarely coincident, and the exhausted state of blood in the former need never be confounded with consumption. The anæmia induced in the more advanced stages of phthisis is not here alluded to; the blood gets drained of its red particles in tubercular as in all chronic affections; but we are now treating of the earliest and premonitory symptoms." (P. 93.)

In summing up the chapter on the premonitory stage, the following hints are given as guides to prognosis, although at so early a period caution is enjoined:

> "*A.* The insidious form of phthisis, scarcely announced in its beginnings, is more likely to become chronic than that which invades with acute access of fever.
>
> "*B.* The acute form is ushered in with much febrile action, and considerable gastric disturbance; the chronic rather by a gradually progressive emaciation than by fever.
>
> "*C.* Severe sweating is indicative of rapid cases.
>
> "*D.* Very slow emaciation is most frequently seen in the phthisis of advanced life, the changes in the nutrition of the body generally proceeding to a great degree before local disease is established.

"*E.* Phthisis succeeding acute affections, as fever, rubeola, &c., generally belongs to the tolerably rapid form of the disease, but not inevitably to the most acute.

"*F.* Phthisis after pregnancy, and during lactation, is generally rapid.

"*G.* The rheumatic temperament tends to chronic rather than to acute phthisis.

"*H.* The multiplication of hereditary tendencies in a single case has more a diagnostic than a prognostic value. The *form* of phthisis which prevailed in other members of the same family may help us to estimate the probable issue of the case. If two or more brothers have had, say chronic phthisis, the chances are in favour of the development of the same type of the tubercular disorder; if a brother or sister has been struck down by the acute variety at the same age, we may fairly anticipate a similar progress.

"*I.* The absence of assignable cause for any case presenting the features of early consumption is a feature favorable for chronicity. When neither individual temperament, previous disease, nor hereditary tendencies are present, the prognosis for a slow rather than an acute form of the disease may safely be formed.

"*J.* Where any of the conditions rarely concurrent with phthisis are observed, as emphysema, &c., the consumptive disease is often latent and generally slow." (P. 97.)

Passing over the chapter on acute phthisis, we find in chapter viii a description of the first stage of ordinary phthisis. Mention is made of the two forms of tubercle, of the various theories of the morbid action, and of the possible terminations of this stage. Of the latter, four are acknowledged, viz.—1. Absorption, with complete recovery, general and local. 2. Toleration of the deposit for an indefinite time, and no further deposit. 3. Absorption of the animal portion of the deposit, the earthy remaining. And 4. Softening, the most common of all terminations. Chapter ix is devoted to the consideration of those fortunate cases in which absorption occurs, and arguments, authorities, and cases, are given in support of such a possibility. To it is appended a summary of the conditions most favorable to this happy prognosis. In chapter x the signs and prognostics of the first stage of ordinary phthisis are taken into account. In eighty cases, observed with peculiar care, Dr. Pollock found the following relative frequency of the physical signs:

EIGHTY CASES.	No. of cases.
Dulness of percussion observed in	All.
Deficiency of respiratory murmur	69
Bronchial voice	48
Rough inspiration	47
Expiration prolonged	47
Mobility of chest-walls lessened	41
Flattening of chest-walls	38
Tubular quality of respiration	32
Dry crepitation	17
Wavy inspiratory sound	15

Wavy inspiration, to which special attention is given, is regarded as due to "pleural thickening and slight adhesions, associated with a deposit of crude tubercle near the surface of the lung;" and in the first stage, with certain limitations, it is a prognostic of chronicity, and even with signs of more advanced disease in the lung it may be accepted as indicative of a tendency to prolongation and chronicity. As regards the position of tubercle, when it occupies the base of the lung the course of the disease is generally chronic. Hæmoptysis, in the author's experience, is indicative of early softening, but of prolonged second and third stages.

Intensity of fever and absence of complete remissions, with gastric symptoms of severity, taken together, point to rapid phthisis, and in this instance the extent of physical signs is of less moment than the symptoms.

It is a common belief that a gain of flesh invariably corresponds to improvement in the local disease. On this head Dr. Pollock remarks—"The converse of this proposition, when applied to the later stages of phthisis, is not always true, namely, that in cases where flesh is regained the tubercular disease in the lung improves *pari passu.* It may do so, but more commonly the lung affection remains stationary, while the nutrition of the body increases." This is very consistent with our own experience.

In chapter xi, "Chronic first stage; the deposit tolerated," two forms of disease are described, one in which neither systemic disorder nor symptoms of local disease exist ("latent" phthisis), but only a lowered vital power; the other in which signs of irritation of the lung, along with recurrent febrile attacks, denote disease of a very chronic type. In the former variety the disease may remain smouldering for years, its subject performing the duties or pursuing the pleasures of ordinary life, and sometimes reaching old age, at which period disease may resume its activity. When the disease does recur after, it may be, a long period of quiescence, it is generally acute, and carries off the victim. In these cases of prolonged first stage the systemic disease has ceased, the local irritation has ceased, and digestion has not been impaired at the invasion of the disease. The temperament associated with these cases of arrest in the first stage is the lymphatic, with its feeble pulse, slow circulation, inactive body; mental powers good, but placid; intellect vigorous, but not brilliant.

The second variety of chronic first stage is attended by frequent febrile attacks, probably due to successive slight deposits of tubercle. The symptoms of lung irritation, with occasional blood-spitting and sub-crepitant râles, are present. The average *observed* duration of such cases was 47·48 months, but this is probably far short of the real duration. This chronic form of disease most prevails between the ages of twenty to forty-five.

At the close of chapter xii, which is devoted to "Calcareous deposits in the lung," the following propositions in reference to that subject are laid down:

> "I. Cases of cretaceous transformation occur generally in old age, but are also numerous in adult life. They are seldom met with before twelve years of age.
>
> "II. There is no early condition from which the change may be prognosticated with certainty; but it appears most frequently in cases where tubercle has been scattered rather extensively through the lung, as indicated by the physical signs.
>
> "III. A general harshness of the respiratory tones, and a diffused sharp crepitation, often resembling pleural sounds, with much impairment of the movements of the chest and considerable flattening, are the ordinary physical signs.
>
> "IV. A repetition of chalky, waxy, or bony expectoration, with subsidence of the symptoms, frequent slight hæmoptysis, dyspnœa, occasional severe exacerbations of cough, with fever, alternated with periods of comparative cessation of the more distressing symptoms, form the chief features of the case.
>
> "V. The secondary disorders, as diarrhœa, are long delayed.
>
> "VI. This form is generally non-hereditary or accidental.
>
> "VII. It is generally extremely prolonged, and a safe prognosis for duration may be offered when once the condition is fairly recognised."

Space does not permit our doing more than naming the headings of the following chapters. These are—xiii, "Chronic second stage;" xiv, "Chronic third stage;" xiv, "Strumous Phthisis;" xv, "Age a modifier of Phthisis;" xvi, "Rheumatism—Gout—Disease of the Heart;" xvii, "Emphysema of the Lung with Phthisis, Asthma, Chronic Bronchitis;" xviii, "Menstruation, Pregnancy, Lactation;" xix, "Hæmoptysis;" xx, "Gastric symptoms, Diarrhœa, Fistula;" xxi, "Pneumothorax," xxii, "Hereditary influence."

And here we would pause, having come to two chapters which, in our opinion, merit special mention and commendation. These are entitled—xxiii, "Preventive treatment of Phthisis—Infancy, Childhood;" xxiv, "Preventive treatment of Phthisis—Occupation." So sensible, judicious, and practically important, is the advice given in these chapters, that we think it would be extremely desirable to have them published as a reprint in the pamphlet form. More than once since receiving the book have we put it into the hands of parents whose children had on one or other side an hereditary claim to consumptive disease. We feel confident that, did the public know more of the hygienic conditions required by those inheriting tubercular tendencies, we would have in the middle and upper classes of society fewer such cases to treat. The topics involved in such considerations, viz., the management of children so predisposed, their food, the use of baths, clothing, value of exercise, light, ventilation, education, including the consideration of competitive examinations and violent exercises, stimulants, tobacco smoking, and, finally, the choice of occupation—these, we say, are topics which directly concern the public at large, and for this reason the information which Dr. Pollock has so clearly and pleasantly given might advantageously be placed within their reach.

The chapters upon the treatment of the disease when once established contain no startling novelty. In specific treatment Dr. Pollock is evidently not a believer. He regards the disease as composed of too many complex conditions to be met by any single omnipotent remedy. At the same time he fully appreciates the value of remedial agents addressed to the varied morbid actions the sum of which constitute the disease. We think few will gain-say the truth of the following remarks:

> "That medicinal agents have certain special powers is not to be denied. It is their capability of remedying certain diseases in their nature complex, and not their ability to effect alterations in given conditions of vital actions against which we contend. Thus, a deficiency of red corpuscles can doubtless be remedied by iron, and the fatty tissues can be nourished and a richer chyme prepared by the use of fish oils; and old deposits of inactive character can be rendered more susceptible of organization, and, therefore, of removal; and absorption of the plastic exudations of inflammation can be stimulated and hastened, and excessive secretions can be restrained, and so waste prevented. Well-selected tonics may be addressed to and will reach the muscular apparatus of the hollow viscera, and the nervous system be strengthened by the same remedies. Or, by a numerous and valuable class of agents, the direct sedatives, we can lower nervous susceptibility to reflex impressions, lessen the respiratory requirements of the system, diminish nervous waste, and vastly assist in restoring our patient to a condition favorable for the resumption of the vital actions which constitute health."

In dealing with the different stages of the disease, Dr. Pollock recommends the adoption of the following principles in reference to the constitutional disorder:—1. The furtherance of healthy blood changes. 2. The maintenance of full respiratory action. 3. The elimination of morbid matters from the system. 4. The supply of the largest amount of the most nutritious food which can be digested. In the first stage of the disease he is not averse to local depletion, subject, of course, to certain reservations. He holds that the great error of the present day is the exhibition of oil in all stages of the disease. It can do no good when local irritation is present, but must be held in readiness against the first pause in the activity of the local process. During the stage of softening counter-irritation finds its special application, and the form which Dr. Pollock thinks best is the strong solution of iodine. In the third stage a seton, an issue, or a perpetual blister, are applicable when cavity is fully formed and stationary in its limits. In all stages the remedies for relief of cough, diarrhœa, perspiration, &c., find their time of application, and their exhibition must be left to the judgment and skill of the practitioner.

And now we trust our readers can form some opinion of the character of Dr. Pollock's book. The extracts we have given and the rough analysis we have attempted are, we hope, sufficient to prove that the work is one of much value. It ought to be on the shelves of every physician or general practitioner. Its author has shown us the principles by which our cases of consumption may be grouped. In this respect he has struck out a new path. His conclusions as to detail may or may not be correct. They are founded upon a vast number of tabulated facts, and it is only by the further observation and record of facts that their truthfulness is to be ascertained. The future will return its verdict. Meanwhile we cordially commend the work to the attention of the profession.

REVIEW IX.

1. *Saint Bartholomew's Hospital Reports.* Edited by Dr. EDWARDS and Mr. CALLENDER. Vol. I.—*London,* 1865, pp. 827.
2. *Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital.* Vol. II, 1865, pp. 415.

FOR a long series of years Guy's was the only hospital which, by the publication of a yearly volume of reports, gave forth to the world any systematic representation or digest of the vast experience gathered by the medical staff within its walls, or by former pupils distributed in various parts of the country.

Our readers will remember, that in a previous number we hailed with much pleasure a similar production by those connected with the London Hospital. The ancient and munificently endowed charity dedicated to St. Bartholomew, not willing to lag behind, now instructs us by the issuing of a goodly first volume of the same kind. Every one of our large metropolitan hospitals (and why not also our provincial ones?) ought to follow in the wake, and we already have certain assurance that within a very short time we shall be able to mention at any rate one other of our metropolitan hospitals which has determined to follow an example so worthy of imitation. We had hoped in this notice to review the recent (the 11th) volume of the 'Guy's Hospital Reports,' in addition to those placed at the head of this list. For lack of room we must consider its contents in our next number, and confine ourselves at the present to the consideration of its compeers.

Commencing, then, with the notice of 'St. Bartholomew's Hospital Reports,' the article coming first therein is one by Mr. Paget. It is not a very lengthy one, and is, we regret to say, the only one from his pen. It is entitled, "Cases of Chronic Pyæmia." The author begins by stating that cases are not of uncommon occurrence to which the above name is appropriate, resembling those of the well-described and typical pyæmia in most ways as regards mode of formation, causation, symptoms, &c., but differing from them in their duration, in extending over many weeks and months continuously, in having relapses, and in being "often free, at least in their later stages, from all severe general disturbance of the health, and from nearly all risk of life." After discriminating between this kind of case and those which at the onset are really acute instances of the nature ordinarily described, but which assume a very slow fatal course, he proceeds to describe the details of six highly interesting cases, from the consideration of which he arrives at the following conclusions:

> "It is not rare to meet with examples of disease presenting the essential characters of pyæmia, but much slower in progress, and much less severe and perilous, than those from which pyæmia is usually described.
>
> "These cases are frequent enough to justify the general use of the names 'chronic' or 'relapsing' pyæmia.
>
> "They are more rare among the instances of pyæmia following wounds than among those occurring in diseases.
>
> "The local evidences of chronic are, more often than those of acute, pyæmia, seated exclusively or chiefly in different parts of the same tissues; they are more frequent in the trunk and limbs than in internal organs, and when seated in the veins are most frequently found towards the close of the disease.

"The nearest affinities of chronic pyæmia are with rheumatism, through gonorrhœal or urethral rheumatism; with simple or single abscess-formation after fever; with hectic fever; yet, with very rare exceptions, the diagnosis from all these is, in practice, clear.

"The prognosis in chronic pyæmia may usually be very favorable, especially when there are long intervals between the successive local manifestations of disease and no evidence of serious pulmonary affection. The slower the pulse and breathing, and the less the sweating, the greater are, in general, the probabilities of recovery.

"The usual treatment of chronic pyæmia may be with good food, patient nursing, a moderate use of stimulants and tonics, and an abundance of fresh air. The value of this last condition was strikingly shown in Case V. The influence of the liquor potassæ in Case IV deserves consideration. Its curative power seemed clearly proved; and I suspect that a part of its reputation for causing the absorption of tumours is due to its influence on morbid deposits imitating tumours, such as existed in that case." (P. 12.)

II.—*On the Diagnosis of Systolic Endocardial Murmurs, whose point of greatest intensity is at or near the Left Apex of the Heart.* By JAMES ANDREW, M.D. Oxon.—In this paper the author mainly concerns himself with such murmurs heard within the mitral area as have their seat at the ventriculo-auricular orifice, being regurgitant in character, and with such as are merely ventricular, *i. e.* produced within the ventricle and by the "onward" stream of blood, the distinction between these two classes of murmurs being dwelt upon. In considering this distinction the value of certain physical signs of disease of the heart is discussed, viz., those of "hypertrophy, intensification of the second sound in the pulmonary artery, and the degree in which the murmur is audible posteriorly at or near the inferior angle of the left scapula."

A table containing 100 cases, classified according to the presence or absence of the physical signs just enumerated, is given. As regards the murmur heard posteriorly, the author considers this the most important sign of all. If it is not audible in the region alluded to, the author believes that it seldom indicates regurgitation.

Of the above-tabulated cases thirty-two are given in detail, but the number of the above in which post-mortem verification was feasible was very small. The table indicates the number of cases in which irregularity of pulse, thrill and musical sounds existed, and those in which rheumatism had occurred.

We find it utterly impossible to condense into a sufficiently small compass the particulars of the cases which are detailed. It must suffice that we commend their careful study to such as have access to the volume, and to give the deductions which

Dr. Andrew draws from the statements made in his paper. They are as follows:

> "1. That of systolic murmurs, audible at or near the apex, a large number, 34 per cent., do not indicate mitral regurgitation, or for the most part any very serious lesion.
>
> "2. That the decision as to the regurgitant or non-regurgitant character of a murmur rests principally upon its presence or absence posteriorly.
>
> "3. That intensification of the second sound in the pulmonary artery, as a gauge of the obstruction to the blood stream on the left side of the heart, is of a great value, but that in estimating it there are certain precautions to be taken.
>
> "4. That the occurrence of a non-regurgitant systolic murmur may be explained by changes on the inner surface of the ventricle, or by dilatation of its cavity leading to undue tension of the chordæ tendineæ.
>
> "It would have been easy for me to have employed an equal number of selected cases, which would have apparently established these propositions in a far stronger and more conclusive manner than those actually tabulated may be held to have done. For every case requiring explanation, one might readily have been substituted in which no such necessity would be felt. If my argument seems to be imperfect, it is because I have preferred the honest statement of facts with all their difficulty and complexity, the cases being taken strictly in the order in which I met with them, to the specious demonstration of a foregone conclusion." (P. 34.)

III. *Brief Notes of the Surgical Practice of the Hospital.* By George A. Callender and Alfred Willett.—This paper makes mention of a great number of cases, which are noticed in a somewhat disconnected manner, and of which the consideration is a little perplexing by reason of the different groups of cases not possessing separate headings or being marked off from their neighbours. Among other classes of interesting cases we have cases of diseases and excision of joints, diseases of bones (among which are two curious cases, one of exostosis, in which the pedicle of the new growth had been fractured, and one of a bony tumour in the posterior triangle of the neck, raising the subclavian artery, and apparently attached to the last cervical vertebra. (It would be a point of interest to know if the pulse was affected on the affected side.) Cases of hernia, of "hurt joints," fractures, abscesses, cancer (including cancer of the entire tongue, which was successfully amputated),[1] hydatid cysts, cartilaginous and myeloïd tumours, hæmatocele, wounds from accidents, &c., the details of amputation of an arm owing to a burn, and also of a presumed case of rupture of the trachea.

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[1] In one case a large cancerous mass found in the leg of a child, aged eighteen months. In another, one of cancer of the bodies of several vertebræ, complete paraplegia was produced? We should like to have been told whether the spinal cord was encroached upon.

IV. *Hypertrophy and Prolapse of the Tongue occurring during Convalescence after Scarlatina; removal of the protruding portion with the Ecraseur and Scissors, with, ultimately, a good result.* By Bowater J. Vernon.—The patient was three years of age, the lower jaw was mis-shapen, the incisor teeth depressed and everted, and the disfigurement very great. The operation was performed by Mr. Paget, and, owing to subsequent growth of the tongue, had to be repeated. The shape of the jaw became much more natural, and the tongue ceased to be a disfigurement. Allusion is made to the cases of a similar nature collected by Mr. Humphry.[1]

V. *Practical Observations upon Tumours of the Pelvis and neighbouring parts complicating Pregnancy and impeding Labour, with illustrative cases.* By Robert Greenhalgh, M.D. St. And.—The author regards such tumours solely in a mechanical point of view, and his observations have reference merely to the diminution of the pelvic and abdominal cavities during pregnancy and parturition produced by tumours. The details of fifteen cases are given, and they are also arranged in a tabular form; their nature and the results being as follows:

> "One case of thrombus of the labium and vagina; four cases of tumours of the ovaries; four cases of tumours of the uterus; one case of tumour of the rectum; one case of tumour of the kidney; four cases of extra-uterine fœtation.
>
> "Eight of the patients reached the full period of uterine gestation; in the remaining seven pregnancy terminated at some period between three and seven and a half months of gestation.
>
> "In four cases labour set in spontaneously; in two cases labour was induced artificially.
>
> "Four patients were delivered by the natural efforts at the full period; one at the third month.
>
> "In fourteen cases the labours lasted for periods varying from five to thirty hours.
>
> "Of the fourteen children, eleven presented with the head, one by the feet, one by the arm, and another by the breech.
>
> "Twelve were males, four females.
>
> "Turning was had recourse to in four cases, but was completed only in three, the fourth being subsequently terminated by the Cæsarean operation.
>
> "Two were delivered by the forceps.
>
> "Five of the mothers and eight of the children were lost; of the former, three died from exhaustion and two from hæmorrhage." (P. 98.)

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[1] 'Med.-Chir. Trans.,' vol. xxxvi.

Dr. Greenhalgh closes his communication (which is to be continued) by observations, illustrated by instances which have come under his own experience, upon cases which simulate pregnancy, and others in which extra- and intra-uterine pregnancy may be obscured by absence of signs and symptoms.

VI. *Restoration of the Lower Jaw after its entire Removal. A case, with Remarks.* By Thomas Smith.—The patient, aged thirty-five, was a lucifer match-maker. The necrosed jaw was removed by dividing it at the symphysis and dragging the two halves separate. The dead bone, from which the soft parts had extensively receded, was pulled away (the entire jaw, excepting the left condyle) without the use of the knife, denuded of soft parts, &c. Two weeks after the operation the patient ate fish, and in a month meat. The frame-work of a new jaw formed and increased, but he was frequently the subject of attacks of dyspnœa. He died one night with symptoms of suffocation, and death was attributed partly to œdema of the upper part of the larynx and partly to falling backwards of the tongue (which seemed to have caused the dyspnœa), which, owing to the lower level of the newly formed jaw, was brought nearer to the hyoïd bone and the upper opening of the larynx. A plate is given, showing the relative position of the old and new jaw, and another showing the microscopical appearances of the latter; and the relation of the muscles and nerves to the partly formed new jaw are described in detail. The soft tissues around the periosteum of the old bone appear to have furnished the nidus of the new bone. Alluding to the fact that in phosphorus-necrosis of the lower jaw there is abundant formation of earthy material, whilst in the same disease of the upper jaw no such deposit is formed, the author suggests that this difference in reparative power is exactly analogous to that shown by a compact living bone (to which in structure and disposition of periosteum the lower jaw is comparable), as distinguished from a cancellous or solid bone to which the upper one is comparable.

VII. *On the Local Effects of Blood Poisoning in relation to Embolism.* By William S. Savory, F.R.S.—The writer, after cursorily noticing the views which have been held as to the formation of what are generally termed "secondary abscesses," specially alludes to those (now most prevalent) of Virchow and his school, who look upon them as due to the mingling with the blood of minute solid particles, for the most part of disintegrated fibrine, which, arriving at the capillaries or smaller vessels, block them up, and thus become the foci of changes which lead at length to the formation of pus. After rather minutely describing the mode of formation of the secondary abscesses, and the various stages through which they pass, first of all petechial spots or patches, then congestion and stagnation, perhaps extravasation, inflammation, degeneration, and subsequent changes in the products, &c., he asks the question—" What causes are competent to produce such mischief?" In the first place, can it be produced by mechanical obstruction—by emboli? In other words, what effects result from the introduction of disintegrated fibrine into the circulation? Mr. Savory then relates the particulars of nine experiments on dogs and rabbits, in which solid materials were injected into the veins, disintegrated fibrin, oxide of zinc, &c., the animals being killed at various subsequent periods and the different organs examined. The effect produced was the formation of spots and patches in organs from the accumulation of blood, blocking up and distending the vessels, the central parts of the masses being most marked, at which places the pustules of the injected substance were found. He proceeds to ask whether similar effects, patches of congestion and spots of stagnation, could be produced by any other agency? What effects will follow the injection into the blood of putrid fluids which contain no solid particles? He then gives the details of cases in which fluid, rendered putrid by the maceration therein of flesh filtered from solid particles and by putrid liquor amnii, was injected into the veins (chiefly the femoral) of dogs, rabbits, and cats. In these cases very much the same appearances were met with in the viscera as in those wherein particles of fibrine or oxide of zinc had been injected into the veins, appearances in either case depending on stagnation and congestion, the result of blocking up of capillary vessels, caused mechanically in the one instance, and in the other by interference with those relations which exist between the blood and the tissues, in either case the lungs being principally and particularly affected, owing to the total or partial arrest of the mischievous matter in the lungs.

In Mr. Savory's experiments the stage of suppuration was never fairly reached, a fact attributable to the subjects of observation being healthy, also to the fact that the power of repair is much greater in animals than in man. The more extensive and profound effects induced by the injection of putrid fluids than by solid particles arise—

> "Not only because fluids are more readily diffused throughout the mass than solids, and more liable to escape in parts through the first set of capillaries, and then to set up further mischief, but chiefly because, beyond the local effects produced by stagnation in the capillaries, they no doubt induce changes in and so damage the constitution of the blood itself."

Mr. Savory afterwards observes that—

> "The results of the injection of putrid matters into the blood vary according to the amount and intensity of the poison. But I think all the marked conditions induced may be referred to an alteration of the blood, whereby its circulation is hindered."

He quotes the experiments of Gaspard, Majendie, Sedillot, Panum, and others, regarding the effects of injecting putrid fluids and foreign substances into the circulation; and further relates additional experiments of his own, showing the effect of injecting oil and pus into the veins. He concludes as follows from his experiments, altogether amounting to twenty-seven:

> "That spots and patches of congestion and stagnation, with, perhaps, ecchymosis in the lungs and other organs, may be produced by injection into the veins—
>
> "Of minute particles of solid matter suspended in water, which can undergo in the blood no decomposition, or induce any chemical change, and consequently can only act mechanically.
>
> "Of putrid fluids which have been previously carefully filtered, and so deprived of any solid particles.
>
> "Of pus.
>
> "That in either case the local effects are the same. The most critical examination fails to detect any distinction in the engorged portions of tissue produced by these different means, except that when solid particles which undergo no change are employed they may be discovered in the midst.
>
> "The action of fresh pus in producing these effects is mechanical. Either its cells, or the clots which it may form in the blood, become impacted in the capillaries. When putrid, no doubt it acts in both modes. Pus, then, has no peculiar or specific action when present in the blood. If putrid, it will act like putrid matters generally do; or, its globules may, under certain circumstances, like other foreign particles, become arrested in and block up the capillaries, producing effects already described." (P. 134.)

Again—

> "In all cases (of pyæmia) stagnation and congestion first ensue. The subsequent changes, whether towards resolution, suppuration, or gangrene, are determined by—
>
> "The action of; the morbid fluid or obstructing substance; the constitution and state of health of the individual."

VIII. *Two Cases of Poisoning by Mercuric Methide.* By George N. Edwards, M.D. Cantab.—In both these cases the patients were assistants in the laboratory of St. Bartholomew's Hospital, and had inhaled the poison;[1] one proved fatal and the other became idiotic. All working in the laboratory at the time suffered to a certain degree from inhaling the poison. The symptoms produced, unlike those from any other cause, are given, and are of great interest. In the fatal case the appearances presented by the brain are said to correspond with those found in acute mania, a disease often mentioned as an effect of mercurial poisoning.

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[1] Mercuric methide and ethide are varieties of corrosive sublimate, the former having the formula $HgMe_2$.

IX. *Cases in which large Arteries were tied during the year* 1864. By ALFRED WILLETT.—These are ten in number, operations on vessels of less size than the brachial, and all amputation ligatures being omitted, and are arranged in two groups, the first including those in which the operation was performed for diseases of the vascular system, one only being for disease other than aneurism; the second comprising those in which the operation was necessitated for the suppression of hæmorrhage. We find it impossible to epitomise these cases. The reader will find two pages of instructive observations made regarding the points of interest which they present.

X. *The Minute Structure of the Human Kidney.* By REGINALD SOUTHEY, M.B. Oxon.—The kidneys examined by Dr. Southey were those of the ox, the sheep, and of man. His paper is divided into a discussion of the glandular and vascular elements, and whether the vessels be followed as regulating the order and arrangement of the gland-tubes or these last as necessitating a particular capillary distribution, the plan upon which both are fitted together is fully and carefully described.

In the upper medullary portion of the kidney the straight tubes are shown to be arranged in bundles, with branches of vasa recta between them.

As the straight tubes pass up into the cortical portion these bundles are still further divided from each other by downward prolongations of the tortuous tubules, so that sections of the cortical portion demonstrate alternating columns of straight and curling tubes, which run side by side of each other close up to the kidney-capsule.

The Malpighian bodies always lie in the tortuous tube territories.

The capillary networks arising, for the most part, and only not quite entirely, from the Malpighian tufts, are very finely meshed in the tortuous tube districts; they are much longer meshed when running between the straight tubes in the straight tube columns or territories.

The minuter points upon which the author lays some stress are the thickness of the walls or membrana propria of the tortuous tubes, the granular and highly nucleated character of their contents, which only become distinctly cell-formed lower down in their course, the interosculations of the tortuous tubes, and the great variations in size which they experience.

The glomeruli are undoubted terminations by blind extremities of the urinary tubules. Dr. Southey has been able to isolate the tubule, with its Holland-flask-like termination, from the kidney of an ox, and this by a process which he has detailed.

The vascular tuft at the end of the several afferent arteries is described as clasping the ends of the tubes, as the claw of an eagle might hold a globe, the muscular sheath of the artery being blended into the substance of the globular enlargement of the tube, the external connective-tissue sheathbeing continued over this, and pinching round its neck.

The vasa recta are believed by Dr. Southey, as by Kölliker, Henlé, and others, to originate from a re-collection of capillaries which are derived mainly from the lower set and larger glomeruli, and are argued not to spring from separate vasa ascendentia stems, as Professor Virchow has supposed. Dr. Southey imagines that he perceives a physiological explanation of the purpose of the Malpighian tufts, in that they govern the blood supply, regulate its speed, and obviate any undue pressure; he supposes that they effect this object through the extension of the muscular coat of the afferent artery into their capsules. He evidently regards the vasa recta as nothing more than large capillaries, in which the blood is directed to flow in broad channels beside the emulgent urine tubes, just where these are thinnest walled and most distinctly lined with clear epithelium. The conditions here attained are certainly those most favorable to an exosmosis of watery fluid, and here, as the author shows, Henle's looping prolongations of the tortuous tubes dip down to pick up the water which is required to dilute the more solid urinary excreta.

The pathological confirmations of the correctness of these views we believe to be tolerably strong.

XI. *Remarks on the Operation of Excision of the Knee-joint.* By HOLMES COOTE.—After certain prefatory remarks regarding the mode of growth of various kinds of bones in man and animals, and illustrations showing the dependence of such growth on the due exercise of nerve-power, the writer proceeds to point out that nerve-power in a limb may be lessened and nutrition arrested by direct violence, dwelling on the great injury which a limb must sustain by the extirpation of such a joint as that of the knee. "Hence," he observes, "the operation of excision is, as a rule, unsuited for young growing persons in whom joint disease is certainly not a fatal affection;" and concludes, from the statistics of operations performed at St. Bartholomew's Hospital in 1864, that "affections of joints, especially in the young, are for the most part curable, and do not require so serious an operation as either excision or amputation;" protesting "against the general propriety of the proceeding as a method of treatment." The great dangers from the operation "consist primarily in the nerve-shock to the patient; and, secondarily, in the consequences of extensively opening the cancellous tissue of the bone, and in the prolonged period of convalescence. In all cases the limb is withered, small, and weak; in many instances it is useless, or nearly so. When performed on the young, growth only adds to the consequent deformity; when performed on the adult, the attendant dangers are immeasurably increased."

Mr. Coote gives the details of two cases in which he performed this operation on the knee, stating the reasons which induced him to have recourse to it. In both cases the symptoms were due to an accident; both patients were country bred, and healthy, also tall; therefore the difference of length in the limbs would be permanently limited to the immediate effects of the operation. Moreover, symptoms of synovitis were absent, and the disease was limited to a portion of bone which admitted of removal.

The paper is concluded by a tabular list of ten cases of resection of the knee which had been performed in the hospital since August, 1863, showing a mortality of one in five.

XII. *Report of a case of Ichthyosis, with Congenital Malformation of the Aorta.* By WILLIAM CHURCH, M.B. Oxon.—The affection of the skin was partly papiliform and partly squamous, and was remarkable as being confined to the left half of the body. Moreover, the mucous membrane of the left side of the mouth, palate, and tongue, was the seat of papiliform growths, and the molar teeth were wanting, the gum being hollowed out into a broad and deep furrow, studded with hard papillæ. The patient died suddenly, and immediately below the left subclavian artery the aorta was found to be suddenly narrowed, the foramen ovale being very large. No cyanosis or marked cardiac symptoms had existed during life.

XIII. *On Tumours containing fluid Blood.* By W. MORRANT BAKER.—The author's observations mainly concern those tumours in which the blood is variously altered, and not spontaneously coagulable. After relating examples of this kind of swelling, and alluding to their description by authors, Mr. Baker proceeds to give reasons for supposing that the fluidity of the blood is due, not to the fact of its never having coagulated, but to the fact of its having become coagulated and then rendered fluid again. He draws the following conclusions:

1. That although the coagulation of blood extravasated in the cellular tissue, or in cysts or hæmatoceles, is probably delayed, yet it does occur after a longer or shorter period.

2. That in many of these cases the whole of the blood does not escape at once, or within a short time, but that the hæmorrhage continues for a considerable period, although the loss of blood is only small—too small to cause wide-spread extravasation or bursting—too great to allow the disintegration and absorption of that which has already escaped to proceed at a greater rate than does the effusion of fresh blood.

3. That the fluid obtained from these tumours is, in many cases at least, not simply uncoagulated blood, but a mixture of serum (derived probably, not only from extravasated blood, but secreted also by surrounding parts) with blood-cells, diffused colouring matter of cells, and disintegrated fibrine.

4. That, besides the constant but small effusion of blood, there is a constant absorption, although not sufficient to keep pace, or to do more than keep pace, with the effusion.

Special reference, on several occasions, is made to the observations of Dr. Burrows and Mr. Prescott Hewett in connection with the subject of this communication.

XIV. *Observations upon some points in the Anatomy of the Supra-renal Capsules.* By Dyce Duckworth, M.D..Edin. (Two Plates.)

§ 1. *The naked eye appearances, as modified by age and other apparently normal circumstances.*—The author desires to direct attention to the fact that the supra-renal bodies are small before puberty, and increase with the other organs of the body. They are large about the second month of fœtal life, but become smaller towards the close of that condition. Their preponderance in the embryo is almost peculiar to the human subject; the sheep is, however, an exception in this respect.

Before puberty they are small, firm, and sometimes translucent. Fatty matter is in minimum quantity during this period. In adults they are less firm—never, in health, translucent—and the fatty particles are increased. Hence their colour varies according to age; their size is hardly appreciably altered in advanced life. No facts have been adduced in proof of the statement that the capsules are larger in swarthy than in white races, and the author quotes dissections by Mr. Turner, of

Edinburgh, and himself, which correspond with some by Cruveilhier,[1] to disprove the assertion.

§ 2. *Appearances presented on section of the supra-renal capsules.*—It is shown that the aspect of sections varies according to the amount of blood contained in them. In the description of the relative anatomy of the cortical and medullary portions the author confirms the statements of Kölliker[2] and Harley.[3] The variety of appearances is due mainly to the condition of the medulla, which, when congested or partly decomposed, is of a chocolate-brown colour, but when anæmic presents a pearly aspect. Several explanations as to the formation of cavities in the capsules are given, and their existence is held to be due partly to decomposition and partly to rough manipulation in removing the organs.

§ 3. *Nature of the expressed juice of the cortex of the supra-renal capsules.*—Pale cells, round, oval, or polygonal, are met with in the juice of the healthy cortex, together with myriads of bright yellow refracting molecules and granules. The cells have well-defined walls, and contain one or two nuclei of varying size. They occur singly and in linear groups, attracting to their walls granular matter. In their general aspect they much resemble lymph-corpuscles. Other smaller cells, non-nucleated and containing granular matter, are also visible. Ecker considers the larger cells to be nuclei, and believes that the investing granular mass is undergoing development into a cell-wall. The author maintains that he has never found a perfect cell of this kind, either in the juice of the cortex or *in situ* in a section through the part.

H. Frey,[4] in his article on the capsules, gives a drawing (after Ecker) illustrating his views on this point.

§ 6. *The relation between the cortical and medullary portion of the supra-renal capsules.*—The fibrous stroma of the organ is shown to be continuous from the cortex to the medulla. Its fibres are accompanied by bundles of nerves and blood-vessels. The inter-columnar vessels anastomose freely at the inner limits of the loculi, and then pass on to form the medullary plexus. The columns of the cortex abut boldly against the more delicate medulla, and the pigmentary line in the former serves to define the limits more clearly.

§ 7. *Nature of the expressed juice of the medulla.*—The juice of the medulla closely resembles that of the cortex, but it possesses fewer refracting granules. The author has never succeeded in finding cells of an angular form with processes passing off from their sides, as described by Kölliker.[1]

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[1] Cited by M. Martineau in his 'Traité de la Maladie d'Addison,' p. 7. Paris, 1864.

[2] 'Manual of Human Microscopic Anatomy,' Kölliker.

[3] "Histology of the Supra-renal Capsules," by George Harley, M.D. 'Lancet,' June, 1858.

[4] Todd's 'Cyclopædia of Anatomy,' Article "Supra-renal Capsules," translated by Dr. Brinton.

§ 8. *The medullary matrix and its contents.*—The fibres of the medullary stroma radiate from stellate masses to form meshes. These masses contain granular matter which imbibes pigment, *e. g.* carmine, &c. There may sometimes be seen in them what appear to be nucleated cells. These bodies have been taken for multipolar nervous ganglia by various observers, and hence has arisen the belief that the supra-renal bodies are important nervous structures. From the fact that nerve-branches are seen to ramify and course beside these masses, without in any way communicating with them, the author believes they are not nervous structures, but regards them as part of the stroma of the organ.

The medullary cells are not disposed formally as in the cortical loculi, and there is no evidence of a basement membrane within the meshes; hence the contents lie free, and in direct connection with the vascular and nervous textures.

The plates exhibit in coloured lithographs the appearances, under different magnifying powers, of sections made in various directions, and of the juices in man and some other animals.

XV. *Cases recently under Treatment.* By THOS. WORMALD.

XVI. *Cases from the Wards.* By ROBERT MARTIN, M.D. Cantab.—It is impossible to give any satisfactory analysis of this communication, which consists of the careful record, out of the experience of seven months' practice in the wards of St. Bartholomew's Hospital, of several "pathological puzzles and diagnostic dilemmas," all being cases of long standing, and with obscure symptoms. We find, first of all, a case of medullary cancer of the brain and spinal cord and their membranes, a case which affords many interesting points of speculation; then a case of renal dropsy consequent on scarlatina, attended by convulsions and sudden blindness, in which recovery took place, and in which typhus fever supervened and was recovered from; then cases of aneurysm of the arch of the aorta and of the abdominal aorta; then one of cancer of the mediastinum and left lung, with entire obliteration of the bronchi, mistaken for empyema; followed by one of enlargement of the spleen, liver, thymus and lymphatic glands, with increase of white blood-cells, and purpura, &c.; and, lastly, one of cirrhosis of the left lung, and amyloïd degeneration of the kidneys, spleen, liver, intestines, supra-renal capsules, and bronchial glands.

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[1] Op. cit., p. 423.

The notes and observations which accompany the relation of most of these cases are highly interesting and practically instructive in a clinical point of view, and well worthy of careful attention.

XVII. *Case of Acute Necrosis of the Tibia; death from Pyæmia.* By William S. Savory, F.R.S.—This case is intended to illustrate the relation in which the spots and patches of congestion or ecchymosis sometimes found in the lungs or other organs after death, in cases of blood-poisoning, stand to the secondary abscesses of pyæmia.

XVIII. *Note on Hippuric Acid.* By William Odling, M.B. Lond., F.R.S.—The object of this somewhat abstruse paper is to show the various and discrepant notions which chemists have entertained as to the chemical and intimate constitution of this substance, and to reconcile them with one another.

XIX. *Fractures injuring Joints; Fractures interfering with the movements of the Wrist and with those of Pronation and Supination.* By George W. Callender.—In this paper the author brings much knowledge and experience to bear upon the subject of fractures of the radius, and mainly of the carpal extremity, the mode in which they occur, especial reference being made to impaction, the degree of displacement and its direction, and the influence of these changes upon certain joint-movements. No fractures, he observes, require more careful examination, as many of them must have a comparatively unfavorable issue however good their management may be, and in many the recovery would have been better had the original injury been accurately determined and the right treatment adopted. He remarks that their precise nature is not clearly defined, and their unsatisfactory after-consequences are not, as a rule, sufficiently impressed upon the patient. Out of the 155 fractures of the forearm of all kinds under treatment at the hospital in 1864, no less than seventy-eight of this class of injuries existed. Mr. Callender illustrates his observations on the various kinds of fracture of the wrist-bones by numerous apposite cases and notes of specimens in the various London hospital museums. Fractures of the shaft of the radius are also considered. The nature of this paper is such that we cannot condense it, but recommend it to the practical surgeon.

The concluding article of the volume consists of—

XX. *Hospital Registration.* By **George N. Edwards**, M.D. Cantab.—It consists, in addition to prefatory remarks explaining the mode of registration adopted, &c., and of the means by which it is carried out, of tables containing a summary of annual statistical reports for the five years 1860 to 1864 inclusive, and a synopsis of the number of cases admitted for each disease for each of those years.

We now pass on to analyse—

Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital. Vol. II.

The first communication in this volume is from the pen of Mr. John Adams, and consists of observations "On a case of Dry Gangrene, following Ulceration and Occlusion of the Femoral Artery." This was a case described as being in all respects like to senile gangrene, except that it occurred in the person of an emaciated woman, aged 43. She had been in great poverty, and received a kick in the upper femoral region, which led to inflammation and abscess, at the bursting of which excessive hæmorrhage, endangering life, took place. This was arrested by pressure and ice, and the wound healed. Dry gangrene, however, of the foot supervened, and when Mr. Adams saw her no pulsation could be felt in the femoral artery or in any artery below the seat of injury, nor at the cicatrix of the wound; but after this the femoral artery could be felt beating. Mr. Adams, after resorting to good diet and tonics for a time, amputated the limb below the knee, no secondary hæmorrhage occurring. The only vessel which required a ligature was the anterior tibial artery, and all the vessels excepting this one were found to be plugged up by coagulum. The heart was pale and flabby, and the liver was fatty. Mr. Adams considers that the cause of the coagulation of the blood in the smaller vessels (in addition to the arrest of circulation in the main artery), which was, no doubt, the origin of the gangrene, was dependent upon the weak condition of the heart, by which the blood was not transmitted through the anastomosing vessels with force sufficient to maintain the circulation in the extreme arteries, a condition which obtains in old people who are the subject of gangrene. The stump healed very slowly, and abscesses formed in the course of the lymphatics. The patient sank and died. Mr. Adams relates briefly the history of three other cases of gangrene, in which he had performed amputation.

Mr. Adams's cases are followed by a short paper by Mr. Curling, entitled, "Observations on the Treatment of Painful

Cancer of the Rectum, by establishing an Anus in the left Groin." After some prefatory allusion to some remarks made by the author in the 'Lancet'[1] respecting the prevention, or mitigation, or retardation of many of the most distressing symptoms of this disease, illustrated by three cases, he relates at length the case of a gentleman aged 52, who suffered from cancer of the posterior wall of the rectum, which eventually caused a communication between the bladder and the rectum, and in whom an artificial anus was made in the left loin. The patient died five weeks after the operation, which, owing to his objections, had been too long postponed; but he was relieved from the severe attacks of abdominal pain consequent on overloaded bowels, and was saved the misery of fæces passing into his bladder. Mr. Curling gives a table of all the cases of lumbar colotomy in diseases of the rectum which he has performed or assisted in, being ten in number; and of these, seven recovered so far as to survive one month after the operation. He observes—

> "The success which has attended lumbar colotomy in persons weakened by long-continued organic disease, and also by want of nourishment when obstruction existed, shows that the operation is not so formidable and dangerous as is commonly supposed. That an artificial anus in the loin is not a constant source of trouble and annoyance, as many persons imagine, is amply shown by perusal of the cases in the table, and by others in which the patients have lived for years after the operation. The bowels act at regular intervals, and the escape of flatus and feculent matter at other times can be prevented by a well-adjusted pad and bandages. When diarrhœa occurs, or when unhealthy gases are generated, then annoyance is experienced. The chief inconvenience liable to occur arises from prolapsus of the upper part of the colon, which occasionally happened, and gave trouble in the case detailed in this paper." (Page 12.)

Mr. Curling draws attention to the use of iodine lotion (ʒiv of the compound tincture to ℥viij of water), in rendering the feculent discharges from the artificial anus inodorous and keeping the wound sweet.

On Liquid Diffusion in relation to Physiology and Practical Toxicology. By H. LETHEBY, M.B., Ph.D., &c.—This paper has for its object an exposition of the experiments and views of Professor Graham on that interchange which occurs between two different aqueous solutions when brought into contact, either directly or by placing the lighter over the heavier in the same vessel, or indirectly, as by means of a membrane or other non-porous medium; and a review of the practical applications of the phenomena of this liquid diffusion in the analysis of the fluids and tissues of the body, especially in cases of poisoning. After describing the various modes of investigation adopted by Graham, and the results obtained, Dr. Letheby observes that a careful consideration of facts and the clássification of substances according to their diffusive power enabled Graham—

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[1] January, 1865.

> "To perceive that all those bodies which are endowed with great diffusive mobility are characterised by their faculty for crystallizing, while those of the opposite class, the comparatively fixed substances, are remarkable for an absence of this property. He noticed, moreover, that the several members of the latter class are distinguished by their being gummy or viscous in their liquid condition, and by their forming gelatinous solids with water. This is the case with the gums, albumen, gelatine, dextrine, starch, tannin, caramal, and animal and vegetable extractive matters, as well as certain mineral substances, as the hydrates of silicic acid, allumina, peroxide of iron, peroxide of tin, &c. And as gelatine, or rather its pure principle, collin, might well be considered as the type of these substances, he has named them colloïds, in contradistinction to the former group, which he has termed crystalloïds. As regards liquid diffusion, therefore, there are two classes of substances, viz., the crystalloïds or crystallizable bodies, which are most diffusive, and the colloïds, or uncrystallizable, which are the least diffusible." (Page 20.)

Later on he draws attention to the fact that Graham was able to procure the colloids in their two conditions of liquid or soluble and gelatinous or insoluble, which has led to his knowledge of many remarkable properties possessed by them. Alluding to the liability of the colloidal forms of matter to molecular change, Dr. Letheby observes that this property—

> "Is so characteristic of them that it may be regarded as the most striking peculiarity of the class. It may, indeed, be said that the special quality of a colloïd is its susceptibility of change, and that its existence is a continued metastasis."

Further on he observes—

> "And here the question presents itself, whether the colloïd molecule may not be constituted by the grouping together of a number of smaller crystalloïd molecules, and whether the basis of colloidality may not really be this composite character of the molecule? It is worthy of notice that the equivalent of a colloïd is always high, although the ratio between its elements may be simple, and it would seem as if this high number is attained by a repetition of a smaller. In the case of the colloïdal form of silicic acid the equivalent in one of its combinations is thirty-six times greater than that of the crystalloïd silicic acid; and the small proportions of lime and potash in gum indicate that the colloïd gummic acid has a very high equiva-

lent; so also with colloïd bases, the large proportion of iron and alumina in certain colloïd compounds of these bases is proof of their high colloïdal equivalent." (Page 25.)

This view of the poly-molecular structure of a colloid Dr. Letheby suggests as explanatory of the attributes possessed by the colloïd forms of matter for the performance of organic processes, all the plastic elements of living beings being either liquid or pectous colloïds, and the processes of organization being accompanied by a change of the liquid into the pectous solid. He observes—

> "Perhaps the very first step in the progress of matter to organization is the passage of it from a crystalloïd to a colloïd condition, and the last act of its physiological existence is the backward movement of it from a colloïd to a crystalloïd, there being a period of functional activity when it assumes the highest forms of pectous colloidism. These metastases of matter cannot be sudden, and therefore the protraction of chemico-organic changes may not unreasonably be referred to the gradual manner in which such molecular movements occur." (Page 26.)

He comments on the fact that most of the phenomena of secretion and absorption exist under the conditions known to be necessary for the passage of a substance by liquid diffusion through a colloïd membrane; the saline matter of the secretions which are mixed with the crystallizable *débris* of worn-out tissues passing by dialysis through the colloïd structures of the secreting organs, leaving their liquid colloïd associates, albumen, fibrine, &c., in the blood and tissues behind them, and in absorption the very diffusible crystalline bodies passing readily into the circulation. All saline compounds, and especially the highly diffusive acids, Dr. Letheby observes—

> "Have a strong savour when they are tasted, for they pass freely through the colloïd membrane of the tongue; but colloïd substances, as gelatine, gum, and albumen, have no such savour, for they cannot reach the sentient extremities of the gustatory nerve." (Page 26.)

At the same time it is pointed out that the plastic elements of our food, albumen, gelatine, and fibrine, are incapable of dialysis, and it is suggested that the act of digestion is not merely a liquefaction of these substances, but—

> "A molecular movement of them into a crystalline form, and that the highly diffusive acids and salts secreted so freely into the stomach during digestion are concerned in the change."

Part of the difficulty in explaining these physiological processes, of course, vanishes, if we suppose the alimentary mucous membrane to be porous, as are other textures. The coagulation of the blood and the formation of plastic and fleshy tissues and colloïd mineral deposits in animals are explained by the facility with which a liquid colloïd becomes pectous or solid, the change being molecular, and not chemical. The formation of blood-crystals is attributed to the diffusion of the colloïd colouring matter of the blood, which has acquired a crystalline character through the colloïd substance of the blood-corpuscles; and many of the bloody discharges, sordes, &c., in diseases, as also the uncoagulated state of the blood in poisoning, are supposed to be owing to the assumption of a crystalloid character by the colouring matter of blood. Dr. Letheby notices the various practical applications of the phenomena of liquid diffusion made by Graham, Liebig, Marcet, &c., but more especially the discovery thereby of poison in the fluids and tissues of the body, as illustrated after Graham by Tardieu, Cossa, Reveil, Grandeau, Lefort, Redwood, and others, the advantages of this process in detecting poison being its readiness, the avoidance of the danger of introducing foreign substance, and the yielding of the poison in a very pure condition.

On the Poisonous Properties of Essence of Mirbane, or Artificial Oil of Bitter Almonds (Nitro-benzole).—In this communication, also by Dr. Letheby, the history, properties, mode of formation, &c., of this singular and powerful agent are recounted, and five cases of poisoning thereby collected together; the results also of experiments therewith on dogs and cats are given, from which it appears "that a poison may be retained in the system for many days without showing its effects;" and that the poison "may be changed into an entirely different substance," *i.e.* into aniline, which subsequently becomes oxidized and changed into mauve or magenta. The processes for discovering nitro-benzole and aniline in the body after death are minutely given.

Circumcision as a remedial measure in certain cases of Epilepsy, Chorea, &c. By N. Heckford, M.R.C.S.—Five cases of cerebral disease, wherein masturbation was practised, are related, in which this operation was resorted to by the author as an intended remedy. After reviewing the results, Mr. Heckford points out that they "are not very startling!" Still, he thinks "the partial success of the plan of treatment adopted is quite sufficient to warrant a repetition of the same in similar cases."

Notes from a Clinical Lecture on a case of Hydrothorax, with *short extracts from other Lectures given at the bed-side on other cases, and some general observations on Pleuritic Effusions.* By Dr. FRASER.—Among other points of importance dwelt upon in this paper is the fact that dyspnœa in all chest affections, and especially in pleuritic effusions, is a very deceptive sign, as "it may be very intense when there is a very small effusion, and absent altogether when the pleural cavity is full;" and this fact is explained by assuming that dyspnœa "arises from the lung being hindered, from any cause, in following up the expanding and uninjured chest-wall," and "not from the absence of air from the air-cells," for if so the dyspnœa would be most intense when the lung is most compressed, whereas it ceases when this condition arises.

Dr. Fraser points out the means of diagnosis between pleural effusion on the one hand and tubercular and other consolidation, abscess of the lung, enlarged liver, &c., on the other. He considers the question of thoracentesis, and the reasons which have been urged in favour of and against this operation, concluding strongly in recommending its more frequent adoption. He says "the whole bearing of the question now tends to an operation in all ordinary cases."

Remarks on the Statistics of Stimulation versus Depletion are also by Dr. FRASER.—In these observations the author gives certain statistics of the London Hospital, as to the annual average quantity of wine employed and number of patients under treatment, and the average mortality during a period of five years (from 1860 to 1864, inclusive). He says—

> "From the foregoing statistics it is evident that a steady rise in the employment of stimulants in the treatment of disease and injuries is still going on; and, whatever be the cause, we may rest assured that the practice is imperative and needful, for it would be a monstrous assumption that a whole staff could be blindly following an objectless routine."

On some cases of Hernia, and on the chief cause of Deaths after Hernia Operations. By JONATHAN HUTCHINSON, F.R.C.S. —The observations contained in this communication were made in a clinical lecture. The conclusions drawn from the consideration of cases related, &c., were as follows:

1. That peritonitis very rarely occurs before either the operation is performed or taxis effected.

2. That it is the common cause of death after operations, and is even now and then met with after successful taxis.

3. That two forms occur after operations, one when the sac has been opened, due to the incision, handling, &c. (direct

traumatic); the other, whether the sac has been opened or not, due to the return of damaged intestine into the peritoneal cavity.

4. That the knuckle of intestine which has been damaged by strangulation, but is yet much short of actual gangrene, may originate the several forms of peritonitis.

5. That the knowledge of the true cause of the commonest form of post-hernial peritonitis explains why deaths often occur even when the sac may not have been opened. That it also points to several important practical conclusions:—(*a*) The extreme value of time in a case of strangulated hernia. (*b*) The necessity for opening the sac and examining the gut, in cases of long strangulation. (*c*) The propriety of retaining the damaged gut in the sac, if found in a bad condition. (*d*) The propriety of adopting, by anticipation, the treatment for peritonitis if inflamed bowel have been returned.

On a case of Fibro-Plastic Tumour of the Scalp. By JONATHAN HUTCHINSON, F.R.C.S.—The details of this case are accompanied by two lithographs, showing the growth *in sitû,* also a section of the same, as well as its microscopical appearances. Excision of the tumour was performed, a common screw tourniquet having previously been placed round the head, just above the ears, with a compress over the temporal artery, and during the operation no arterial bleeding occurred. Exfoliation of a part of the surfaces of the bone occurred, and eventually, as it appears, the patient died of osteitis and pyæmia.

On Lupus and allied Diseases, their Diagnosis and modes of Cure. By JONATHAN HUTCHINSON, F.R.C.S.—In this paper true lupus is diagnosed from "strumous ulceration," what is described as "phagedænic lupus," "syphilitic ulceration," Paget's "rodent ulcer," and also notice is made of certain cases in which psoriasis has superficially the character of lupus, but from which it differs by the thin scab covering the parts, and the cicatrix character of the middle of the patch. He discards the distinction often made between lupus "exedens" and "non exedens," as this difference only depends upon the anatomical peculiarities of the part affected and upon the degree of ulceration. As to the causation of lupus, Mr. Hutchinson attributes the disease, in addition to local damage, to a general constitutional state connected with the so-called tuberculous diathesis, having no relation whatever with a syphilitic taint. He considers the doctrine of the spreading of lupus "by contagion from its own edge, as a cardinal one, and of extreme importance in guiding our treatment." Of this treatment the most important is that which is local, consisting of escharotics, chiefly undiluted chloride of zinc and carbolic acid, care being had to give tonics, good diet, and air, &c., according to general condition and the want of these. Instructions as to the mode of using these applications are given.

Mr. Hutchinson proceeds to discuss the nature and treatment of strumous ulceration of the skin, syphilitic serpiginous ulceration, tertiary syphilitic phagedænic ulceration, and rodent ulcer, concluding with a statistical summary from notes of seventy-seven cases of lupus which had come under his notice during a period of eighteen years.

On the Results of the Blister Treatment in fifty cases of Rheumatic Fever. By HERBERT DAVIES, M.D.—Most of these cases had been under the author's personal superintendence, and from a consideration of these cases he draws the following inferences:

1. The blister treatment, if adopted early, when the local symptoms are most marked and the constitutional disturbance most evident, and before any physical signs of endo- or pericarditis are developed, undoubtedly, in a large majority of cases saves the heart from inflammatory mischief.
2. Relief to the local pains is rapidly and permanently obtained.
3. Constitutional effects are manifested in—
    (*a*) The fall of the temperature in the body as measured by the heat in the axilla.
    (*b*) The diminution in the rapidity or character of the pulse.
    (*c*) The altered reaction of the urine.
4. Convalescence from the disease is soon established, as shown by the early return of the appetite and strength.
5. The mode of treatment, though bold, energetic and decisive, is not remarkably painful or to be dreaded, as proved by the evidence of the patients who have been subjected to the plan.

New Facts and Opinions as to Inherited Syphilis. By JONATHAN HUTCHINSON, F.R.C.S.—This communication occupies above fifty pages of the volume. So many points are discussed therein, and so many cases related in illustration, that we find it utterly impossible to give our readers any adequate notion of the paper as a whole. Among other considerations and questions discussed therein are the following:—The possibility of transmitting syphilis to the third generation; the extent to which the diathesis of inherited syphilis can protect against subsequent contagion, or modify its results; severe organic disease, diseases of bone and forms of lupus, &c., in connection with inherited syphilis; the distinction between secondary and tertiary syphilis; the phenomena of latency in respect to inherited taint. Considerable space is devoted to the consideration of the teeth as a means of diagnosis. Instead of following the author into these various subjects, we must content ourselves with referring the reader to the paper itself.

In a short communication from Mr. Hutchinson which follows the above, entitled "Why did not the Hospital Phagedæna occur in Gloucester Ward?" Mr. Hutchinson takes occasion to state the opinion that this disease "depends for its spread on a special contagion," and "that it is absurd to consider the prevalence of hospital phagedæna as proof of anything essentially unhealthy in the condition of the building."

Note on Compound Fracture opening into the Elbow-joint, and on the removal of Cartilage in Excision of the Elbow-Joint and in Operations about Joints.—By C. F. MAUNDER, F.R.C.S.—At the conclusion of his observations the author remarks, "I believe prolonged confinement in the unwholesome atmosphere of a hospital to be more prolific of blood-poisoning than the presence of open cancelli in a wound."

Case of Pistol-shot Wound of the Chest. By C. F. MAUNDER, F.R.C.S.—After considering the details of the case, Mr. Maunder observes that from it the following facts may be deduced:—1st. The lung may be traversed from apex to base by a pistol-ball, and the blood extravasated be venous. 2nd. The lung may be traversed as above stated, and for several hours there shall be no bloody sputum, and even should the patient survive forty hours there may be very little sanguineous expectoration. 3rd. An isolated patch of emphysema will indicate the locality of the concealed missile.

The above short communication is followed by two others from the same author, the one entitled, "Note on the Importance of a Differential Diagnosis of Chancre," in which the rapidly ulcerating phagedænic tertiary sore is recommended to be treated with free iodine in addition to iodide of potassium; the other entitled "On the Significance of Secondary Enlargements of Lymphatic Glands."

Account of the Dissection of a complicated example of Hernia without a Sac. By JOHN COUPER, F.R.C.S.—This case is the sequel to a case related by the author, in the first volume of these reports, of femoral hernia in which no sac was present.

In the present instance the rupture was a large scrotal one, in which the cæcum and a piece of colon, partially divested of peritoneum, occupied the outer parts and front of the scrotum, and were not contained in a sac. Behind and internally lay a large sac filled with strangulated small intestine. The author discusses the varieties of this form of hernia. He also observes—

> "It is interesting to note that, in the cases in which the absence of abdominal obstruction has caused the hernia to be overlooked and "herniotomy to be omitted, the result has usually been less disastrous to the patients than might have been anticipated. They have usually recovered after gangrene and perforation of the gut and the temporary discharge of fæces through the wound." (P. 243.)

And this is ascribed to the permeability of the gut, by which vomiting and long fasting are prevented, and to the non-implication of peritoneum, by which there is less danger of peritonitis.

Case of Punctured Fracture of the Base of the Skull from a Fall on a Spike. By the same author as the last-mentioned paper.—The broken-off end of the spike remained for a time hidden in the wound, and was only removed with difficulty. The spike had entered under the apex of the right mastoïd process, traversed the internal ear, and driven several irregular masses of the anterior surface of the petrous bone (where it forms the roof of the semicircular canals) through the dura mater, deeply wounding the posterior part of the middle alæ of the right cerebral hemisphere, the brain being softened and streaked with pus. Muscular spasm of the muscles on the right side of the body, with paralysis of the left limbs and face, and hyperæsthesia of the right side of the face, were produced. Delirium and coma preceded death. The paper is illustrated by two lithographs.

Cases of Malingering, with Remarks. By W. B. Woodman, M.D., Resident Medical Officer.—Almost all the illustrative cases are taken from the practice of the London Hospital, and are very varied in character, including interesting cases of retention of urine, poisoning, and chorea. Dr. Woodman aims chiefly to establish the following propositions:

1st. That nearly all those who feign diseases or accidents are really in some way or other in ill health; or, in other words, that a basis of truth underlies most attempts at malingering.

2nd. That the difficulties in the detection of these cases are only to be fully met by a comprehensive knowledge of real diseases, and by a constant study of human motives and actions.

3rd. That the resources of our art have been of late years so enriched by the microscope, ophthalmoscope, stethoscope, and laryngoscope, and other aids to physical diagnosis, and the apparatus of the chemist, that the detection of frauds is now much easier than before. But that—

4th. In all cases of doubt it is better to assume "for the time that the patient's statements are true, than to run the risk of maltreating or neglecting a real case of disease."

Dr. Woodman follows up the above communication by a contribution to the "Statistics of Hernia Operations," including facts respecting fifty-five cases operated on in the hospital between October, 1860, and January, 1862, inclusive.

Subsequently we have placed before us the details of twenty-three cases illustrating surgical practice, selected from these sections by students in competition for the gold medal, and these are followed by—

Lectures on Hemiplegia. By J. HUGHLINGS JACKSON, M.D.—These are two in number, and are devoted to the consideration of the different forms of hemiplegia, varied as they are, according to the seat of the diseased action on which they depend. In this way hemiplegic attacks from disease of the spinal cord, the medulla oblongata, pons Varolii, crus cerebri, and corpus striatum or optic thalamus, are differentiated, and many valuable remarks will be found interspersed in the observations made regarding the numerous cases which are adduced as illustrations, and in connection with what the author terms the "medical physiology" of hemiplegia. The lectures are in continuation of the paper which Dr. Jackson furnished on a similar subject in Vol. I of the 'Hospital Reports.' We are glad to see a promise of a further continuation in a contemplated future volume.

Succeeding the above communication we have "Extracts from Clinical Lectures," by Mr. Jonathan Hutchinson, on several important surgical questions; then a "Description of Mr. Maunder's Spray-producer;" followed by "Selected Cases," reported by various gentlemen, and mainly on the "Employment of the Drainage-tube in cases of Suppurative Disease of the Knee-Joint." Finally, a list of the more important preparations added to the museum during the past year by the curator, Mr. Little, and "Reports on the Medical Cases under Treatment in the Hospital" by the medical registrar, Dr. Woodman, and "Statistics of the Major Operations performed during the Year," compiled by Mr. Maunder, close the volume.

REVIEW X.

1. *Report on Cholera.* By Drs. BALY and GULL. *London,* 1854.
2. *Notes on Cholera.* GEO. JOHNSON, M.D., F.R.C.P., &c. *London,* Longmans, 1865.
3. *The Science and Practice of Medicine.* By WM. AITKEN, M.D. Fourth edition, 1866. Griffin and Co., *London.*
4. *A Theoretical Inquiry into the Physical Cause of Epidemic Diseases.* By ALEX. HAMILTON HOWE, M.D. Lond. Churchill and Sons, 1865.
5. *Supplement to the Twenty-fifth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England,* 1864.
6. *Reminiscences of the Cholera at St. George's Hospital in* 1849 *and* 1854. J. W. OGLE, M.D., F.R.C.P., &c. 'Medical Times and Gazette,' 1865.
7. E. MESNET, M.D., *On the Cholera in the Hospital of Saint Antoine, Paris.* 'Archives Générales de Médecine.' February and March, 1866.
8. *Handbook for Yellow Fever, &c., and a Brief History of Pestilential Cholera, and a method of Cure.* THOS. ANDERSON, M.D., F.R.C.S.E., Health Officer, Trinidad. *London,* John Churchill and Sons, 1866.
9. *Clinical Lectures on the Practice of Medicine.* By the late R. J. GRAVES, M.D., F.R.S. Reprinted from second edition, by the late J. M. NELIGAN, M.D. *Dublin,* 1864.
10. *Lectures on the Principles and Practice of Physic.* By THOS. WATSON, M.D., &c. &c. *London.*
11. *Memorandum of the Metropolitan Medical Officers of Health, as a base of common action in cases of Localities visited by Cholera.* 1866.
12. *A System of Medicine.* Edited by J. RUSSELL REYNOLDS, M.D., F.R.C.P. Lond., &c. &c. Art. "Epidemic Cholera," by EDWARD GOODEVE, M.B., &c. &c. 1866.

13. *Researches on Disease in India.* C. Morehead, M.D., &c. *London,* 1856.

14. *Dissertations on Malaria, Contagion, and Cholera, &c. &c.* By Wm. Aiton, M.D., &c. *London,* 1832.

15. *The Cholera at Finglas.* C. F. Moore, M.D. 'Dublin Quarterly Journal of Medical Science,' 1854.

16. *Apparent Causes of Fever and Cholera.* C. F. Moore, M.D., F.R.C.S.I., 'Medical Press and Circular,' 1866.

17. *La Choléra ou Typhus Indien Epidémie de* 1865. *Prophylaxie et Traitement.* Par le Docteur Charles Pellarin. *Paris,* 1866.

18. *Cholera: its Pathology, Diagnosis, and Treatment.* By Wm. Story, L.K. and Q.C.P.I., F.R.C.S.E., &c. *London,* 1865.

19. *Cholera Prospects, &c.* By Tilbury Fox, M.D. Lond., &c. *London,* 1865.

20. *De la Contagion dans les Maladies.* Par M. le Docteur Stanski. *Paris,* 1865.

21. J. Donaldson, M.D., *Cholera at Vizagapatam in* 1861.

22. *Le Choléra et le Congrès Sanitaire Diplomatique International.* Par le Docteur J. P. Bonnafont. *Paris,* 1866.

23. *Brief Remarks on Cholera, &c.* By Robert J. Spitta, M.D. Lond. 1866.

Also papers in *British and Foreign Medico-Chirurgical Review, Dublin Quarterly Journal of Medical Science, Medical Times and Gazette, Lancet, Medical Press and Circular, British Medical Journal, Archives Générales de Médecine, L'Union Médicale, Indian Medical Gazette,* &c., &c.

Although it is most generally supposed that the cholera has always originated in the swamps of Bengal, adjoining the Hooghly and Ganges, there are some who attribute the last outbreak of the epidemic to the Mohammedan festivals at Mecca,[1] where 600,000 to 700,000 pilgrims annually assemble in the city, and upon Mount Arafat, close by.

The mode of life, utterly opposed to all rules of dietetic prudence, pursued by these crowded masses, for ten or fifteen days yearly, entails a number of diseases, to which many are sacrificed. At the annual "feast" of last year, in the beginning of May, the deaths from cholera were variously estimated at from 20,000 to 100,000; many more perished on the return to their various destinations, spreading the disease as they went.

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[1] Since writing the above it has been stated that another outbreak has occurred this year (1866) among the pilgrims, and that many Turkish troops have also perished.

We, however, believe that Mecca served but as the favouring hotbed, where the disease, imported from India, found all the requisites for its intensification and its spread throughout Egypt, Syria, and the Levant generally; and having once gained the seaboard of the Mediterranean, modern facilities of travel soon gave it the speedy means of reaching Italy, Turkey in Europe, France, &c. Paris suffered, and, when it ceased in the French capital in mid-winter, it had effected a lodgment in Normandy, parts of Luxemburg, and the adjacent Rhineland, and, finding suitable countries for its stay, it hovered in these localities till spring. And now, almost daily, accounts reach us of the carriage of the disease by emigrants from Europe to America, as well as of dropping cases, apparently imported from the same source that have already appeared among us.

So wide extended, so fatal, and so awe-inspiring have been the visitations of the cholera, that many inquiries have from time to time been undertaken by Governments, as well as by scientific societies, &c., with a view of throwing light on the apparent first causes, as well as on the predisposing circumstances, which apparently unite in the origination and dissemination of epidemic cholera.

The more we have learned on the subject the greater has become the feeling with us that this disease, scourge as it is, is one of those mysterious and all-important means of civilising the world in the highest sense of the word; in short, that well-being is a requisite for every people, and its promotion by those nations who have the advantages of enlightenment becomes their solemn duty; moreover, every neglect of this duty appears to fall heavily in its consequences upon all.

A conference met at Constantinople, with a view to taking such measures as might be devised to lessen or remove the causes of the spreading of epidemic cholera, and we learn that the French[1] delegates at the Constantinople International Sanitary Conference presented the following propositions, which were adopted:[2]

To break off all communication—the moment cholera appeared among the pilgrims—between the Arab ports and Egyptian coast, leaving the land route followed by the caravan open for the hadjis for their return to Egypt. In other words, the pilgrims would be obliged to perform quarantine, either in the Hedjaz till the epidemic ceased, or in the desert in the caravan route.

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[1] 'Archives générales de Médecine," April, 1866, pp. 503-4.

[2] As stated by the French press.

The carrying out of these measures would require a Mohammedan sanitary commission sent to the Hedjaz, that should forward intelligence of the state of health of the pilgrims; some ships of war to break off the communication by sea, and an organized coastguard to oppose any landing on the Egyptian coast. The commission, assisted, if need be, by other medical men commissioned for this purpose, should telegraph to the local authorities, as well as to the ships of war at Djedah and at Zambo, immediate information of the breaking out of cholera among the pilgrims, and also forward the intelligence to Egypt. In this way the whole preventive machinery alluded to above would be brought to bear on the stoppage of communication with Egypt by sea. A place of quarantine—possibly Tor—would be allotted for all suspected ships to perform quarantine. The caravan should be halted many days' march from Suez, and, being visited by a medical commission, should only be allowed to enter Egypt when all danger would be past.

The evidence given in 1858 by the native medical subordinate of the Madras Medical Establishment, Dr. W. Vencataswamy Naidoo, on the causes of cholera among the pilgrims of Juggernaut,[1] is the more valuable as coming from a native of India, and which we can, to some extent, corroborate from personal experience in the East. The author states that the food of the pilgrims is prepared exclusively by the priests in the temples, and he describes the mode of preparation. The description is almost loathsome; the food is acrid, oily, and often putrid; the drink sour, and the price of food and drink excessive.

> "Pilgrims (he says) thus fed, undergoing every fatigue and toil of long journeys, with privation of food and rest, arrive at their tabernacle half starved or much exhausted, and have a small enclosed room pointed out by the pundahs for the accommodation of twenty or thirty persons. As soon as they have their baggage secured they repair to the temple, washing themselves in different stinking ponds, and, wearing wet or silk clothes (as an undefiled dress), visit the image, eat the various sorts of food with eagerness, taking no notice whatever of its condition, taste, or quality, under a well-impressed idea that such observation would be an act of blasphemy; drink a jumboo-ful of very sour rancid tyre, and feel themselves refreshed and very much satisfied for the first day. But from the second or

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[1] 'Medical Times and Gazette,' 1865.

third day the causes of diarrhœa or of cholera, viz., sudden transition from heat to cold, aliment of indigestible character, acrid food or acrid drinks, oily and putrid substances, and want of free ventilation and drainage, soon operate upon the supposed repenters of sin.

"The streets and houses are impregnated with noxious exhalations emanating from the decomposition of the excrementitious and urinous deposits with which the streets and alleys, fields and plains, are filled during the assemblage of people in great numbers, as well as from the dead bodies thrown out in the fields and in the towns.

"Cholera having thus originated, great alarm and despair are produced amongst the pilgrims; and the fright and despondency on the one hand, and their longings for home, relations, or friends, on the other, act conjointly as depressing agents, rendering their system more favorable to the action of the causes of cholera.

"The ravages made by this disease annually are very lamentable, and most pitiable is it to observe the annual scene at Juggernaut, resulting in deaths in vast numbers, making children motherless or fatherless or altogether orphans, women as widows, men as widowers, and afflicting others with the loss of brothers, sisters, and friends."

Many efforts have been made to discover the exciting cause of cholera, but heretofore without satisfactory result, neither the microscope nor chemistry having added much to our knowledge in this direction.

While many writers have agreed in ascribing to the cholera a mode of propagation of an aërial nature, or at least have contended that it is air-borne, some, as the late Dr. Graves, have shown that it must differ much in this respect from some other forms of atmospheric causes of disease.

That highly gifted physician contrasted in this respect the influenza and the cholera, and we must at once be struck with the difference between the two. In the former almost the whole of the United Kingdom was simultaneously, or very nearly so, attacked, whereas the visitations of cholera have been much less general and of far slower extension. Indeed, we acknowledge the ability displayed by Dr. Graves in his investigations, now so many years since, into this important question, and we feel that time and all the strides of science and art have but served to confirm the truth of his observations, "that cholera seemed regulated by no common physical circumstances, except human traffic and human intercourse." It is not a little remarkable that these observations, noted in days when steam was in its infancy, hold good now, when this agency serves to waft men over thousands of miles in a few days.

For we find, in the late fatal case of the sailor from Rotterdam, that the disease travelled in his system, in the stages of incubation and development, from the Continent across England to Bristol, in a period commensurate with the modern improvements in travelling. It may be asked, Why, if this be so, does not the disease spread with far greater rapidity? The answer appears simply this—In addition to the exciting cause, there is happily necessary also a predisposing condition, and this is often absent, at least among the generality of railroad travellers from the Continent.

In proof of this we find that the crowded emigrant ship or ill-found merchantman, unfortunately, but too constantly, displays that state most favorable to the development of cholera or other zymotic disease, and that it is in such vehicles, and among those who are travellers by or sojourners in such vessels, that the germ of cholera migrates from place to place. Were it otherwise we should almost yearly suffer from cholera carried to us by the overland Indian passengers.

One other point, however, in this respect demands notice. In former visitations the disease advanced by way of the great caravan routes through southern Russia; now, however, increased intercourse from Egypt to Constantinople, France, and the shores of the Mediterranean generally, owing to the operations on the Suez Canal, the greater development of the overland traffic for pilgrims, and the cotton trade with the East, &c., has served to render that the route for the recent advent of cholera to Europe.

Wind, when laden with miasma, undoubtedly has been proved to influence the outbreak and continuance of cholera, while it has also frequently happened that the disease has extended in the very teeth of the wind. In the latter cases investigation has shown that other more potent agencies, such as a line of pilgrims or of other travellers, had been in motion in the direction assumed by the disease.

The important light thrown on the influence of the state and condition of the atmosphere during times of epidemic cholera by the systematic reports of Mr. Glaisher are worthy of remembrance by every practical physician.

> "The three epidemics (he observes) were attended with a particular state of atmosphere, characterised by a prevalent mist, thin in high places, dense in low, during the height of the epidemic. In all cases the reading of the barometer was remarkably high and the atmosphere thick. In 1849 and 1854 the temperature was above the average, and a total absence of rain, and a stillness of air, amounting almost to calm, accompanied the progress on each occasion.
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> "In places near the river the night temperature was high, with small diurnal range, a dense torpid mist, and air charged with many impurities, arising from the exhalations of the river and adjoining marshes, a deficiency of electricity, and, as shown in 1854, a total

absence of ozone, most probably destroyed by the decomposition of the organic matter with which the air in these situations is strongly charged.

"In 1849 and 1854 the first decline of the disease was marked by a decrease in the readings of the barometer and in the temperature of the air and water; the air, which previously for a long time had continued calm, was succeeded by a strong south-west wind, which soon dissipated the former stagnant and poisonous atmosphere."

It would be out of our power to give even an outline of the many conjectures that have been hazarded on the subject of the first appearance of cholera.

Thus, it is by no means clear that it was not known to Hippocrates and Celsus in ancient times, and to Sydenham, as well as other observers, in comparatively modern times; while there is strong reason to believe also that it was known in the British East Indian possessions in the last century. Most writers are content to date its first distinct recognition, with all its characteristics of severity, from the year 1817.

"In 1818 (writes Dr. Goodeve) the western world was startled with the intelligence of the appearance in India of a disease which was ravaging Lower Bengal, and had also attacked the camp of the Marquis of Hastings, then engaged in the Mahratta war, and who was at that time halted on the banks of the Sind, in the upper provinces.

"A new disease, or at least one unknown in such a terrific form, was carrying destruction through all the ranks of the army, both European and native. . . . The scourge appeared in Lord Hastings' camp on 6th November, 1817, and in five days destroyed 5000 men.

"In it in all 9,000 deaths occurred.

"But not in the camp of war only did it cause surprise and terror.

"After having shown itself during the previous months in Mymunsing, Patna, Kishnagur, Chittagong, and some other places, it burst out in August, 1817, in the agricultural province of Jessore, amongst the peasants and labourers of the rice swamps and palm groves. Many thousands were swept away by the pestilence in the course of a few weeks. There might have been such a disease in the midst of past ages, but the memory of living man possessed no vivid or substantial knowledge of it. It burst upon the suffering generation with the violence of an unheard-of plague, impressing all with dread and consternation.

"From this starting-point in India cholera spread east and west, far beyond the bounds of Hindostan. Its appearance in other lands may be traced with tolerable accuracy. From Bengal it spread eastward and southward in the following chronological order. We find it in 1818 in Burmah, Arracan, and Malacca; 1819, in Penang, Sumatra, Siam, Ceylon, and the Mauritius; 1820 in Tonquin, China, and Clima; 1822-23-24, in all China; 1827, in Chinese Tartary.

> "Turning to the west, we find it in July, 1821, at Muscat and the Persian Gulf; in 1822, in Persia, and prevailing there during 1822-23-29-30; and in 1823 at Astracan, without spreading further westward for some years, *i. e.* until 1829, when it reached Orenberg, through Tartary, revisited Astracan in 1830, and from thence started on its course through Europe.
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> "The westward course continued slowly. In May, 1831, it was very severe at Moscow and Warsaw; in July of the same year, at St. Petersburg and Cronstadt; in October, at Berlin and Vienna. In England the first cases showed themselves at Sunderland, in October, 1831, and the epidemic prevailed in the British empire for fourteen months.
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> "It crossed the Atlantic and reached Quebec in 1832. This fatal malady ravaged the whole of Europe, and left that quarter of the globe in 1837, the last place affected being Rome.
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> "Since 1817 epidemics of cholera have been frequent all over India, so that the disease may be said to have been naturalised there, causing a large mortality among all classes.
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> "Besides the first great epidemic above mentioned, the western parts of the world have suffered from two severe visitations of cholera, viz., 1848-49, and in 1853-54. These appear to have travelled from the east much in the manner as that of 1832."

Before concluding the quotation from Dr. Goodeve we must add that we see no reason why he should not have included the recent reappearance of cholera in Europe.

> "Thus, cholera seems to have spread east, south, west, and north from its first birthplace in Bengal, which became but the centre of an epidemic area comprising nearly all the world. It travelled slowly at first, and not continuously, but in irregular waves, checked sometimes, but not destroyed, by winter cold. Neither climate nor season, nor earth nor ocean, seems to have arrested its course, or to have altered its features.
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> "It was equally as destructive at St. Petersburg and Moscow as it was in India, as fierce and irresistible amongst the snows of Russia as in the sunburnt region of India, as destructive in the vapoury districts of Burmah as in the parched provinces of Hindostan.
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> "In both periods at the end of September the temperature of the Thames fell below 60°; but in 1854 the barometer again increased, the air became again stagnant, and the decline of the disease was considerably checked. It continued, however, gradually to subside, although the months of November and December were nearly as misty as that of September."

Our readers will probably, from their own experience, endorse the notes of Mr. Glaisher, while the evidence gathered at home as well as in India and China, and if the very imperfect accounts which have come to hand of the epidemic which has now been for several months in existence on the seaboard of Brittany and that of parts of Germany be correct, all tend to show that cholera clings to a moist atmosphere, especially when in combination with vapoury impurities, such as but too frequently are allowed to pollute the neighbourhood of our rivers and populous sea-coasts. Dr. Goodeve, however, observes, "It is doubtful whether there is any relation between the marsh malaria of a country and cholera."

Among observers generally it has been remarked that rich clayey soils are rather more favorable for the production of cholera than those of a drier nature. We have ourselves known cholera to prove extremely severe in sandy soil, as at Alexandria and Suez, but in each locality there was close proximity to the sea, and much unhealthy impregnation of the sand. That elevation of site has in most instances exerted a beneficial influence against the spread of cholera, there is much evidence to show; that, however, other circumstances may entirely negative the benefit otherwise derivable from elevation, can be readily understood. The records of India prove both these positions.

Various conditions of atmosphere have been regarded by authors as favouring the development of cholera; even opposite states have been mentioned as favouring its development, whether we regard high or low states of the barometer, the presence or absence or alteration in ozone or electricity. It is, however, pretty generally agreed that a high temperature favours its development; and though the cold of a Russian winter has not stopped its progress, the question naturally arises, Have we sufficient proof that the cold of such a winter had fair access to the morbific agent, whatever it may be? And we think this question may be fairly answered—that the habit of keeping the interiors of Russian dwellings at a comparatively high temperature through the winter months, and the habit of using much warm clothing, and a not very scrupulous cleanliness among the lower classes of the Russians, all favour the dissemination and retention among these people, of the germ of cholera, whatever its nature may be.

Heat, however, favours its spread, and adds to the severity of the disease. Dr. Goodeve quotes Dr. Ewart, Dr. Morehead, and Dr. Hugh Macpherson,[1] to show that the greatest amount of fatality has attended epidemics that occurred during the hot months. Dr. Leith, however, mentions that among natives in Bombay the mortality was greater in the three years 1848-52 from October to March than from April to September inclusive. In Europe the fatality has been greatest in the summer season.

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[1] Whose recent work on cholera, we regret to say, came to our hands too late to obtain a notice in connection with this article.

Mr. Glaisher and other observers in England show the connection between the severity of the cholera and a prevailing high temperature. The early morning has been noticed in different parts of the world by many as predisposing to the disease.

In 1853-54 the condition of the atmosphere in England was unusually dry, with the exception of May and December; the rainfall being 18·62 inches, or 5·93 less than the average. Certain conditions of rain and atmospheric moisture, however, undoubtedly favour the spread of cholera, while, on the other hand, rain (if attended by a fresh breeze may it not be) has been known to act in preventing the continuance of cholera.

The evidence on the influence of rain and moisture in the air, of which a *résumé* is given by Dr. Goodeve in the 'System of Medicine,' appears at first sight somewhat contradictory; on further investigation, however, the difficulties appear to vanish when we consider that high temperature, moisture, and a stagnant state of the air, present a combination of influences always found most depressing in its effects on man, as well as highly favorable to the development of malarial agencies. If, therefore, we admit, with many attentive observers, that cholera owes in some way its origin or its favorable development to these agencies, then we see a twofold reason why heat, moisture, and a stagnant state of the atmosphere, should favour the spread of cholera, namely, by predisposing mankind to it by its debilitating effect, and, secondly, by the promotion of malaria. The so-called dry season in Calcutta is that in which cholera is most destructive, whereas in the north-west provinces the disease is generally most severe in the rains.

Now, we would for a moment remind our readers that in ague countries that disease is worst in the hot dry months, because the heat favours the decomposition of the organic matters in the marshy land, which in a wetter season would not proceed so rapidly; again, heavy rain would wash away noxious vapours that would remain suspended in a hotter and more stagnant atmosphere.

The reader will find in the records of the memorable outbreak of cholera in 1817, in that at Kurrachee in 1846, and in the epidemic in the gaol at Meerut in 1861, interesting information on this point.

The labours of British Boards of Health; the reports of the army and navy; of the Indian Cholera Commission; of Dr. Milroy, at Kingston; of Dr. Greenhow; of Dr. Burton, of New Orleans; as well as the experience of Dr. Pettenkofer at Munich, Dr. Buckler at Baltimore, and of the medical officers attached to the army in Bulgaria and the Crimea, as well as of the state of Mecca last year, all corroborate the opinions of those who dwell on the fact that—"An atmosphere impregnated with the products of fermenting excrement is at once the most obvious and most constant concomitant of cholera." In addition to this important and frequent source of foul air, that arising from collections of decomposing animal matter, as seen in the case of the unburied Chinese at Shanghai, as recorded by Dr. Morgan, of H.B.M.S. Euryalus; again in the case of those dead of cholera at Mecca last year, as well as the emanations from the refuse of animals slain to feed the multitudes collected there at the festival of the Kurban Baïram.

Dr. Snow's idea that water frequently became the vehicle by which the cholera evacuations were actually taken into the system, when contaminated water was drunk, appears by no means improbable; at all events, his important contributions to our knowledge in this respect deserve attentive consideration, and that impure water greatly promotes the spread of the disease admits of no doubt; Mr. Simon, Drs. Acland, Sutherland, Wm. Budd, Routh, and others, have all given important testimony on this point.

Dr. Goodeve points out the very injurious influence exerted by bad food in the production of cholera. Dr. Brittain, Mr. Grainger, and many other writers in England, in India, as well as on the Continent, have dwelt on this important circumstance; decayed cheese, bad fish, pork, and other food more or less decayed, has frequently caused severe outbreaks of the disease.

Purgative medicines, not alone saline and hydragogue, but also milder purgatives, have, in the experience of Dr. Goodeve and many others, caused serious mischief during an epidemic of cholera. Sir R. Martin and Mr. Twining have cautioned us against giving any purgative medicine likely to operate in the early morning, *i. e.* about the time that the first symptoms of cholera generally commence.

Sex has scarcely any predisposing influence in the experience of many observers in different climates.

Mr. Grainger found that infancy and childhood up to five, and the ages after fifty years, suffer most from cholera. Dr. Gull has found that individuals under one year and over fifty-five are those that suffer most from the disease, results that accord with the general tendency to mortality in England, as shown by Dr. Goodeve. Dr. Baly, however, drew the distinction between English cholera and epidemic cholera, and showed that the latter was much more fatal in the prime of life than the former. Diarrhœa, like English cholera, was also more fatal to those in early and advanced life.

Very many authors have, like Rousseau, stated their belief that the miasm of cholera requires concentration to render it dangerous. Dr. Graves long ago showed how apt it was to be conveyed by ships; but it is also clear that, if the condition of ships as to ventilation, cleanliness, and consequently of those on board, be good, there is very little risk of their being a means of the carriage of the disease, as we have above pointed out. In proof of this we find much evidence by C. F. Moore,[1] gathered during epidemics of cholera in England in 1849, in Egypt and the Mediterranean in 1848 and 1850, and in Ireland in 1854. His experience of cholera is that the contagion must be very concentrated indeed to affect persons not predisposed to it by other causes, as bad food, debility or exhaustion, bad atmosphere from foul exhalations, damp, &c.[2] We would add that the evidence of its communication by cholera matter in water used as food or by dirty utensils should always be borne in mind, as enforced by Drs. Snow and Budd.

Dr. Bourgogne advocates the idea, also held years ago by Dr. Hartley Kennedy in India, and of which we have met with confirmatory opinions by other men, namely, that cholera is of marsh origin. He accordingly gives tannate of quina in cholera; but Dr. R. de Pietra Santa states that the cholera poison modifies the good effects of quina.

M. Cahen advances the theory, also, that cholera is owing to marsh malaria, and would treat the disease from that point of view.

M. Bonnet regards cholera as not communicable by contact, as spreading wherever it once occurs, and its cause, once produced, as capable of being air-borne.

M. le Docteur Bozzi regards cholera as highly contagious.

That prolongation of residence in India has any effect in predisposing to cholera, or the reverse, has not been as yet satisfactorily determined.

Opinion is divided as to the effect of intemperance on those who may be exposed to the influence of a cholera season.

That the several conditions which prevail at a time of cholera likewise produce influenza, fever, diarrhœa, and other zymotic disease, has been recorded by different observers.

On the subject of the mode of invasion, period of continuance, reappearance, and decline of cholera in localities, a good deal of difference has been observed. In India generally, and in many other places, its commencement has been very sudden; whereas in Britain and France, and in some epidemics elsewhere, diarrhœa has preceded its appearance and continued for months, causing often very considerable mortality. It is not unusual for the disease to begin, cease, and then reappear once or twice again, before finally ceasing.

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[1] 'Dublin Quarterly Journal Med. Sci.,' May, 1852, and Nov., 1854.

[2] Ibid., 1852, p. 318.

From the first appearance of cholera in these countries, at least in recent times, various opinions have been entertained as to its mode of diffusion. That it spreads by means of human intercourse appears quite certain, from experience obtained in every country where it has existed. Several most striking instances are published on this subject.

We cannot, however, positively say that it may not spread by other means also, as seems probable from the fact of its appearance in the retired island, St. Kilda; and other like instances are also on record.

Several circumstances are also, however, on record which go to prove the contagiousness, at least in a degree, of the disease; and recent occurrences on board the emigrant vessels conveying numbers of Germans by way of Liverpool to America go to show that cholera poison is portable either on the persons of travellers, especially those of the humbler classes, or among their luggage.

Again, the fact that cholera has been conveyed from Europe to America by emigrants, and that, as we have learned, the disease has not spread beyond the quarantine establishments in that country, existing within the precincts of those buildings for many months past, goes to prove the existence of cholera contagion, and justifies the resumption of some form of quarantine in the case of arrivals from infected districts.

Several arguments may be adduced in connection with cholera tending strongly to strengthen the faith of believers in quarantine. For our own part, we should be happy to be convinced of security by such means; we fear, however, that it is a disease against which quarantine does not afford positive security; at the same time it is probable that strict quarantine would diminish the probability of its advent and prevent its coming, if we might be allowed the expression, before its time.

In many respects the work of Dr. Baly and Dr. Gull deserves attention, as the most valuable collection of records of cholera. We will very shortly state some leading points from that important work, as well to be borne in mind in the event of an outbreak of the disease. These authors regarded the theory of atmospheric influence as untenable, looking on the cause of cholera as a material substance, and that it was propagated jointly by human intercourse and the wind; they did not, however, regard it as necessarily contagious, nor did they consider it possible to say whether it entered the system by the lungs or by the alimentary canal.

A system of house-to-house visitation they regarded as of the highest importance with a view to sanitary improvements—the promotion of cleanliness in all ships, barges, amongst bodies of troops, the prevention of contamination by the filthy clothes of vagrants, &c. &c.

It advocates the allotment of "houses of refuge," cholera hospitals, or the well-ventilated wards of general hospitals; the destruction of all clothes, &c., belonging to cholera cases, or their thorough purification; and, lastly, the early treatment of all cases of diarrhœa, &c. At the present time we are happy to see that joint action is being taken by the metropolitan health officers, and that they have issued a memorandum for use in localities visited by cholera. It comprises the following headings:—Water—Against the Spread of Cholera—Disinfection and Disinfectants—Speedy interment of the Dead—Special provision for Poor Districts—especially in the way of Sick Wards—Of House-to-house Visitation—Dispensing of Medicines—General Precautions.

M. E. Mesnet considers that, though the late visitation of cholera in Paris has not added much to our knowledge as to the cause and nature of cholera, still he regards the measures adopted for the isolation of the disease as having borne good results. Many interesting facts have also been recorded. Ordinary diarrhœa must not be confounded with that premonitory of cholera. There is much difficulty in determining the commencement of cholera.

If we find the ordinary causes of looseness absent, and debility, malaise, and a degree of vertigo, which latter symptom soon appears, even when the evacuations are inconsiderable, coupled with stools that, from being at first black or brown, become colourless as they increase in frequency (sometimes from the very first evacuation the serous liquid appears poured out by the intestines)—in such cases the evidence of the real nature of the case becomes strong indeed.

At times the epidemic seizes upon the most healthy, but often also the sick, as the patients in our hospitals, are attacked. Derangements of the alimentary canal, defective hygiene, irregularities, excesses, &c.—in short, any debilitating causes predispose to the epidemic

The relations between premonitory diarrhœa and cholera cannot be obtained from hospital records, as many cases of the former disease do not appear at the hospitals.

Cramps Dr. Mesnet regards as the first evidence of the case becoming one of cholera. In some cases of cholera, however, diarrhœa, cramps, and vomiting set in together, or follow each other in extreme rapidity.

A careful consideration of what constituted premonitory diarrhœa—that which preceded the cramps by at least ten or twelve hours—induced Dr. Mesnet to regard 140 out of 213 cases as having been preceded by premonitory diarrhœa. The nervous influences are now, he observes, regarded as far more important than the serous diarrhœa. Dr. Mesnet doubts the wisdom of the definition of cholerine, as given by the Academy (des Sciences); he would rather regard such a definition as expressing what is generally considered as premonitory diarrhœa. Dr. Mesnet coincides with many British physicians, who all along held the opinion that really formidable cholera is not necessarily that which is accompanied by great laxity of the bowels, and whose treatment was in accordance with their tenets. Of 213 cases treated in St. Antoine 140 suffered from premonitory diarrhœa, and but seventy-three from severe cholera from the first, but without premonitory evacuations.

Of the thirty-seven cases of cholera of which Dr. Ogle recorded the post-mortem appearances, fourteen had had diarrhœa preceding the cholera, in five cases no history was obtained.

In seventeen cases collapse and diarrhœa occurred on the same day. The evidence given on this highly important question by the physicians of St. George's Hospital on the one hand, and that of the physicians of the Hospital Saint Antoine on the other, tends to prove that not only do very many cases of fatal cholera occur without premonitory diarrhœa, but also that some of the worst cases are wholly unattended by that symptom. This evidence further corroborates that given by many Indian records, that at the height of an epidemic numerous cases of death occur without diarrhœa, and in a very short time. The abrupt onset of the disease, as Dr. Mesnet observes, denotes cases the most rapidly fatal, whereas many of the cases of epidemic diarrhœa remained without gravity, in spite of the obstinacy of the diarrhœa and its continuance for fifteen or twenty days. Much important evidence has been collected from the writings of Bell, Scott, Parkes, Majendie, Kennedy, and others, by Dr. G. Johnson, to prove that purging is the reverse of a bad symptom in this disease; but we would put our veto to increasing it. In the case of the French hospital, as in that of St. George's, much difficulty was encountered in the attempt to obtain a full antecedent history of many of the cases. Cramps were regarded at Saint Antoine as of much greater importance than diarrhœa.

The algide state was regarded there as "une intoxication plus profonde," for in it, besides the symptoms of the previous stages, we have the circulation, respiration, and temperature, much affected, though varying in degree, and hence designated as the incomplete and complete.

Very true to the nature of this terrible disease is the picture drawn by Dr. Mesnet of deepening phases of the algide state, which also serves to show that the disease, as it recently appeared in Paris, has lost none of the characters it possessed in the previous visitations experienced in western Europe.

Many cases (nineteen) were moribund when brought to Saint Antoine, of whom several perished before they could be placed in bed. Of 213 cases treated eighty-seven died and 126 recovered. Of these, twenty-nine are classed under the head of epidemic diarrhœa, all of whom recovered; of the less severe form of cholera (cholera leger) seven died and forty-two recovered; of cases of "algdité incomplete," twenty-four died and forty-four recovered. Where the latter state was "complete" the recoveries were but eleven to thirty-seven deaths. Of the fatal cases twenty-five in the completely algide state had no reaction, twenty-six presented imperfect and irregular reaction. Six cases presented adynamic-typhoid symptoms, four had meningeal symptoms, one committed suicide, and two contracted other diseases in the hospital.

The author (fairly) congratulates himself on the result of treatment, as (though so many were admitted moribund) of 116 in the algide state fifty-five recovered and sixty-one died.

Almost all the cases of epidemic diarrhœa had 1½ gramme to 2 grammes (22½ to 30 grains) of ipecacuanha in a dose alone or with 30 grammes of sulphate of soda.

The stimulating and alterative ("perturbatrice") action of the ipecacuanha seemed to produce a favorable change in the intestinal secretions, and to have much effect in relieving the circulation ("et réveiller chez les malades une circulation plus large"). "I frequently repeated the emetic for two or three consecutive days." When the evacuations persisted, opiates, and more particularly the laudanum of Sydenham, in doses of thirty, forty, or fifty drops, either by mouth or per anum. In obstinate vomiting ice taken in pieces was useful, and no other drink was allowed.

In the confirmed cholera each case was treated in accordance with its peculiarities and severity.

Shampooing and friction, with or without stimulating applications to the skin, sinapisms in great numbers, and diffusible stimulants internally.

Hot mustard baths, if preceded by active friction of the skin, was more safe and useful than if not preceded by friction. In complete algidity and asphyxia this proceeding was unavailing.

Nettles were used as an external stimulant, but without satisfactory result.

Tea, mint, warm wine, coffee, rum, acetate of ammonia, &c., were used internally. Rum was used cautiously, lest cerebral excitement should arise. When reaction fairly commenced alcoholic stimulants were withheld, coffee and external stimulants being then used.

This author, in treating of the period of reaction, observes that it is not in what we ordinarily regard evidence of superior powers that the power of resistance to cholera resides, but rather in the strength of co-ordination ("synergie") of the fuctions in immediate relationship with organic life.

Reaction he regards as proportionally favorable when all the functions are evenly (parallelement) restored without "precipitation," with the return of the urinary secretion, of perspiration, and when with these changes we have the feeling of health returning. In such cases he observes the patient jumps, as it were, into health. This rapid recovery of favorable cases has been noticed by Sir R. Martin, Mr. Grainger, Mr. Twining, Mr. Rogers, and others. We have seen it very remarkably in many instances, as in one, that of a large and powerful young man, with suppression of urine, who was almost instantly cured, as it appeared, by free venesection; in others on the occurrence, as the author remarks, of free diuresis; in others on the breaking out of warm and free perspiration, &c. A vivid sketch of the effects of a degree of intoxication in three young men is given, in which critical perspirations seemed to terminate both the choleraic symptoms and those of the intoxication. But a warning is also given that intoxication far from, in the great majority of cases, affording any safeguard against cholera, on the contrary, but lays the individual more open to its onset and more feeble to resist its effects. Our evidence quite coincides with Dr. Mesnet's in these respects. Those suffering from alcoholic cachexy, like that of phthisis, of cancer, and of other profoundly debilitating diseases, are very quickly and easily prostrated by the cholera.

Before proceeding with the consideration of Dr. Mesnet's valuable addition to our literature on the cholera, we would observe that the question of the amount of predisposition arising in persons suffering from other diseases is one on which various opinions are held. Dr. Parkes does not consider that such exists; Dr. Gull considers that previous ill-health has but little effect in this way; Dr. Morehead considers that it does. The army returns for India (1861) show that cases in hospital were attacked with cholera in the proportion of 13·5 per cent., whereas of men in barracks but 4·7 were attacked. As Dr.

Goodeve, however, observes, other causes more than mere predisposition probably were in operation in this latter case.

When reaction is arrested it is said to be incomplete, and death may result even after the restoration of a degree of warmth, of natural respiration, pulse, &c.; these favorable appearances in such cases again retrograde towards the algide state. Twenty-six cases perished in this way at St. Antoine's; in some of this sort of case a fatal ending did not occur. Every suitable means of stimulation and of restoring the natural functions should be pursued in such cases. Ipecacuanha, thanks to its combined action of stimulant and alterative, answered well in emetic doses in some of these cases; coffee, warm wine, and frictions, were also used in these circumstances. Critical abscesses also seemed the means of saving life in one case of imperfect reaction. In other cases secondary typhoïd fever, and in others, again, cerebral meningitis supervened.

Of the typhoïd cases there were eleven; of these, eight recovered without complication, and three died, also without the supervention of other disorder; two died suddenly, one had repeated epistaxis, and recovered; one also recovered after a critical epistaxis. Parotitis had commenced in one case that died; suppuration of the parotid gland took place in a case that recovered. Impetigo and abscess occurred in one case that recovered. Of these eleven typhoïd cases, five recovered and six died. Most of them had quinine to the extent of 10 to 15 grs. daily; this treatment was adopted from observing the effects of the remedy in the case of a man who, in the commencement of the epidemic, presented marked symptoms of the typhoïd state in the stage of reaction from algide cholera. He recovered after taking 75 grs. at the rate of 15 grs. daily. Three other cases were also benefited by the remedy, and recovered; two of these had epistaxis occurring critically.

The author speaks of the important part which those critical phenomena, whether of a hæmorrhagic or suppurative kind, played, being the starting-point in the improved symptoms, and evidencing the power of the system to overcome and eliminate the principles that troubled the harmony of its functions. The five who recovered were young and vigorous men, aged respectively twenty-five, twenty-three, twenty-four, twenty-eight, and thirty-two. The cholera seized them in otherwise perfect health. The six who died sank rapidly, two of them suddenly in a state of syncope; one died having incipient parotidean suppuration, which the author had hoped would have proved critical, but he died almost immediately, and the part was found, after death, with the suppuration commencing to permeate its structure.

Besides the typhoïd or adynamic complications, the cerebral or meningeal often occupy a prominent part in the stage of reaction.

Cholera well shows the difference between the nerve-functions of animal life and the expression or features of mental life (les expressions de la vie de relation). In the algide state the patient is restless, cadaverous-looking, with the visceral innervation apparently extinguished, the functions of organic life at a stop, yet preserving his intelligence, and almost to the last able to converse; oppressed in spirit and having his ideas slow, his memory in need of being refreshed, yet when roused and his attention fixed, he gives correct answers to questions. His state is not that of cerebral disease, but a kind of yielding to the general depression with which the organic life is well-nigh overwhelmed.

Cerebral action is slow; sensibility to external stimulants is lessened; hearing is deficient; the sight is feeble, and general sensation blunted. As the algide state passes off, the senses—the means of relationship with the surrounding world—resume their wonted power. At St. Antoine, those recovering were removed to a convalescent ward so soon as their vigour of mind and intelligence returned. Much care should be taken of those cases, as they generally are liable to suffer from exposure to external impressions, and their health, or even life itself, is easily compromised in the convalescence from cholera. Six cases in the period of reaction were attacked with meningitis, beginning on the fourth or fifth day of the reaction; four of them died and two recovered, yet the latter had not the cerebral complication fully developed; leeches were applied to the mastoid processes, and saline purgatives in these latter cases on the first appearance of the complication, with the happy result of a return of the regular stage of reaction. On the post-mortem evidences of these cases there is not much to record; the reader will observe evidence of this also in the cases given by Dr. Ogle. Of the St. Antoine cases we observe that in one (a young man of eighteen) there was slight congestion of the brain-substance; in another, a man aged twenty-one, there were red puncta seen on section of the brain; another, aged forty-seven, had slight rose-tinted congestion of the brain; and the other, aged forty-one, presented some fibrinous plates (*plaques*). In these four cases, cholera had not advanced far, and reaction appeared at first favorable and moderate, then suddenly it became irregular and unsteady: headache; excited eyes; injected conjunctiva; parched mouth, and noisy and excited delirium; picking at the bedclothes; starting of the tendons; contraction and irregularity of the pupils; trismus in some; almost all had stiffness of the neck and stretching of the head backwards; such were the symptoms of these four cases. Two of these cases, on admission, had complete anæsthesia of the whole surface of the body, before the cholera symptoms had assumed an alarming appearance; this led us to expect the possibility of nervous complications, and towards the fourth day of reaction, though other symptoms seemed satisfactory, the head became extended and the meningitis appeared.

The post-mortem appearances gave but little solution of the symptoms. The meninges, transparent and fine, were normal in structure; the cerebro-spinal (céphalo-rachidien) liquid, perhaps lessened in quantity, was of a lemon tint; the surface of the brain, a little dry, was slightly sticky (poisseuse); there was no subarachnoïd œdema, nor diminution of consistence in the brain-substance, but a slight increase of vascularity in the organ, the large vessels of the surface being full without being gorged with blood; in some parts there were networks of small vessels, under which the brain was gray, with a tint of rose, and a little softer than normal; red points were seen on cutting the brain. No trace of meningitis on the base of the brain, or on the upper part of the spinal marrow. In one case, however, there was undoubted evidence of meningitis. In the other three cases we must regard the appearances as those of the dry form of meningitis (méningites sèches), or as cut off in the early stage of the disease, before its full development. Cerebral complications, without actual inflammation, seem to belong to the disturbance of the circulation and loss of nerve-tone; this has been evidenced by the effects of advancing blueness, and by the improvement of the mental state that followed that of the circulation. Idiosyncrasy must evidently influence the phenomena of mental life in the cerebral complications of the reaction stage.

In two cases contractions of the extremities and suicidal impulse occurred in the stage of reaction; and yet in these cases the cholera symptoms had been extremely slight.

In one of the cases the tetanus followed a cold bath, yet the patient recovered, a critical swelling of the parotids, which ended in re-solution, marking the end of the case.

The other case terminated in fatal erysipelas following an attempt at suicide, by repeated blows on the forehead.

In this man's brain was only found the dotted (piquete) appearance which we have often found (observes Dr. Mesnet) in acute delirium of the insane. He goes on to argue that the cholera complicating this case caused what we should not have at all expected in ordinary meningitis. Moreover, in the patient there was no hereditary predisposition, nor any acquired habit to account for the turn the disease took.

Of other ailments which retarded recovery in some cases were diarrhœa and vomiting, which continued long after the disappearance of other symptoms; opiates and subnitrate of bismuth were found the most useful agents in relieving these cases. Pain after food also complained of in some cases, yielded most easily to doses of fifteen to twenty grains of pepsine, taken at the beginning of the meal. Hiccough distressed other convalescents, and generally yielded to electricity applied to the parts of the chest corresponding to the insertion of the diaphragm.

Seventeen cases had been ill of other diseases (almost all phthisical); most of them perished in the algide state; some, however, survived the cholera; it was remarkable that they had a great diminution of expectoration, and consequently of the liquid secretion from the bronchi and pulmonary excavations.

But nine intern cases took cholera; this may please the partisans (observes Mesnet) of non-contagion; for our part, we attribute it to the perfection of the isolation of cholera cases, rigorously maintained, from the commencement of the epidemic, in the hospital Saint-Antoine.

A *résumé* is appended to Dr. Mesnet's work, which we have condensed as follows.

The cholera of 1865 resembled the previous visitations, in its progress and complications. Diarrhœa is not constantly or necessarily a premonitory symptom; frequently it happened that the most grave and rapidly fatal cases did not begin with diarrhœa; the gravity of the evil did not appear to accord with the abundance of the liquid excreta.

The disease is to be regarded as a poisoning, various in intensity, in which the nervous system of the great sympathetic appears to us the first attacked.

Especially regarding the reaction in its uncertainties and complications, at times incomplete and slow, often irregular, or mixed up with the typhoïd or meningitic state, the functions of the cerebro-spinal nervous system are often seen compromised after escape from the accidents of the first period. The crisis under the different forms which came under notice was studied; copious sweats, repeated epistaxis, great collections of pus, manifold abscesses, impetigo, parotid suppuration; and the change was not to be regarded as favorable if unaccompanied by free and sustained gradations.

Including the cases given above, the following summary applies to[1] a total of 407 cases treated in St. Antoine in October and November, 1865.

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[1] 'L'Union Médicale,' Mai 10, 1866.

The premonitory period was marked in two thirds of the cases.

Amid the frightful disorder (affreux) of all the functions of "végétative" life the intelligence is preserved (memory and ideas correct, expression only is slow and painful). Want of harmony of symptoms appears to increase the gravity of the disease—thus, want of accord in the pulse and the temperature of the skin. Again, a predominance of the functional symptoms of the respiration, in cases where the digestive organs appeared little affected, may be regarded in the same light. Generally, cholera left a debility which eminently disposed convalescents to other diseases, as smallpox, pneumonia, erysipelas.

Among the more recent contributions to our knowledge on the history and pathology of cases of cholera, are also those of Dr. J. W. Ogle, which contain details of the post-mortem appearances of thirty-seven cases, as well as notices of the treatment and of the occurrence of previous diarrhœa, and of some other points of interest connected with the cases, from which we extract the following.

The ages of the patients varied greatly; thus the youngest was but two and a half years old, whereas the age of the oldest exceeded sixty. Seventeen of the sick were between twenty and forty years of age; 18 were males, and 19 females.

Eleven of the patients died on the day of admission. The greatest duration of any of the cases under cholera in hospital was ten days. Four were in hospital for other diseases when taken with cholera; viz., 1 for disease of knee-joint; 1 for liver disease, ascitis, and anasarca; 1 for congestion of the lungs following pleurisy; and 1 for cancer of the uterus. As to antecedent diarrhœa, in 5 cases no previous history was obtained on this particular. In 17 cases, collapse appeared on same day as the diarrhœa. In 4, diarrhœa had existed for two or three days before. In 4, also, for three or four days; in 3, for four or five weeks; and in 1, the bowels were "generally relaxed." In 2, the symptoms had existed more or less for a month. In one case sickness and cramps had existed two nights previous to more positive symptoms, but there had been no diarrhœa; and in 1 case there had been vomiting and pains in all the limbs before collapse, but no diarrhœa; neither had diarrhœa come on at all during the course of the disease at any time, clearly showing that the collapse of this affection is not merely owing to excessive discharge of fluids. In many cases it is expressly stated that want of general health, and actual diseases of a lowering character, as fistula, ague, and other diseases, had existed before the attack of cholera; most likely (observes Dr. Ogle) it was the same in many of the cases in which no mention of the circumstance is made. Some retained consciousness to the last, others were even admitted in an unconscious state, or soon became so. Some had contracted, one dilated, pupils; some had no diarrhœa, others but little. In some there was little or no cramp; in two there was actual syncope. Next to collapse, which was common to all the cases, scanty urine appeared the most constant character; yet in one case there was abundance of this secretion. It was often necessary to use the catheter.

Albuminous urine was observed in one case, and casts of the uriniferous tubes of the kidney were also observed.

Calomel was found passed with the fæces in one case. In one case blood was noticed in the stools.

One case complained of a feeling of heat, though in the cold collapse state. In one case the menses came on during cholera; and one poor woman, suckling her infant, did not lose her milk.

A form of urticaria, with a livid colour in one case, and a papular eruption passing on to one of an erysipelatous nature in another, occurred. Consecutive fever succeeded cholera in at least two cases, one being accompanied by stupor, and the other by mania; and in another case pneumonia followed cholera.

Under the head of treatment Dr. Ogle mentions that calomel was given in twenty-two cases; a trial of it was made in large as well as in small doses. Mustard emetics were frequently used; salines were next in order of frequency. The vapour bath was tried in six cases, and hot water baths with or without mustard or with salt were used in four cases. Acetate of lead with opium in five cases. Opium was exhibited in seven cases with or without other remedies.

Sulphur, dilute sulphuric acid (in one case in three-drachm doses every twenty minutes) and capsicum were each given in two cases. Hydrocyanic acid and turpentine were given internally, each in one case; in one case oxygen was inhaled on two occasions, but without apparent benefit; in another the wet sheet, and in two or three cases electro-galvanism was resorted to; in many sinapisms, hot bottles, and friction were used. Once beef-tea and brandy were pumped into the stomach; in others enemata of beef-tea, brandy, milk and eggs were administered; and in a large number abundant stimulants, as brandy, ammonia, chloroform, camphor, &c., were prescribed along with strong beef-tea. The tourniquet applied to the limbs for a time appeared to restrain the cramps in a case in which it was resorted to. Fragments of Wenham Lake ice were swallowed in four cases.

Of post-mortem appearances, those within the cranium, in nineteen cases that were examined, were not of a character that had necessarily relation to the train of symptoms that had preceded dissolution. Slight congestion of the membranes and cerebral vessels—" puncta" on section of the brain, or increased amount of fluid in the ventricles and subarachnoid tissues, being the most usual appearances observed.

Of thirty-five examinations of the organs of the chest there were in sixteen instances congestion of the lungs, in three the results of inflammation were present. Of these latter, two had been recoveries from collapse, but secondary fever arising proved fatal. In the other cases either the post-mortem appearances in the chest were of old standing or did not indicate disease.

Of thirty-seven hearts examined eight were flabby and friable. In one of the cases where syncope occurred the post-mortem report described the heart as " very weak." In thirteen cases the right ventricle, and in six the left ventricle, contained fibrinous coagulum; in six cases no clot was found in the hearts' cavities; in seven the left ventricle was empty and contracted.

Of the thirty-seven cases fifteen presented morbid vascularity of the stomach. In four ecchymosis of blood was met with into or beneath the mucous membrane. In two the inner surface of the stomach was pale and bloodless. In one the lining membrane was thickened, hardened, and ulcerated. The inner surface of the intestines was morbidly vascular in fifteen cases, the colon being so in one case. The latter-named portion of the intestines was described as pale and bloodless in one instance.

In five cases the coats of the bowels were the seat of submucous extravasation of blood; in one case the mucous membrane was said to be soft and pulpy, and in one to be ulcerated. In twenty the solitary glands were obviously and markedly enlarged; and in ten Peyer's patches, or the aguimate glands, were similarly affected.

Dr. Ogle does not enumerate those instances in which, without being actually enlarged, the intestinal glands were simply unusually distinct and visible. In a few cases the solitary glands of the colon were also enlarged. The mesenteric glands were enlarged in ten cases.

In several cases the contents of the stomach and intestines, varying in colour and consistency as they did, were found microscopically to contain a very unusual amount of epithelial cells.

The liver was congested in five cases, the gall-bladder was loaded or full of bile in seventeen cases, only a slight amount of bile in two, and in one no bile but thick mucus.

In thirteen the kidneys were congested. In fifteen the urinary bladder quite empty and more or less contracted; five contained only a very slight amount of urine, and two only contained much urine.

I here exclude cases which presented positive kidney disease appearances, as a granular state, adherent capsules, &c.

In two cases only was the spleen said to have been large and full.

The blood was described as dark and thick in only three cases, in another as very fluid, and in another as very fluid and of a reddish-brown colour.

Dr. Briquet quite recently communicated to the French Academy of Medicine some results of inquiries on the subject of cholera. It has always penetrated from the frontiers north east or south. Never has it commenced in the centre of France. On each visit it has varied in the route it took, depending on the railway development and rapidity of communication.

Some central departments only have escaped the disease; and these, be it observed, are of the poorest and least healthy districts.

As to temperature and climate, it can only be said with certainty that the disease is less fatal, and sometimes ceases in mid-winter.

"We can affirm," observes this writer, that there is a "medical constitution" characterised by derangements of the digestive apparatus always preceding the invasion of cholera in countries visited by that plague.

As to the mode of propagation of the disease, authorities are at variance; however, from the greater bulk of statements we learn that it extends gradually from house to house, from commune to commune, &c.

There is also a great amount of authentic evidence to prove that the disease is transmissible from person to person.

However, observes M. Briquet, the Council (of the French Academy) does not wish to pronounce upon this difficult and obscure subject; it desires only to lay the facts before the profession, leaving to each one to interpret them.

The attempts to inoculate the disease with cholera matter, made by various observers, have not led to positive results; we must therefore conclude that the cholera is not a venomous disease (une maladie virulente).

M. Cazalas, Inspector of the Sanitary Department of the French Army, having fully inquired into the question of contagion or non-contagion of cholera, decides that it is not contagious; he consequently does not advocate quarantine, but the institution of an international sanitary code.

General Conclusions.

The Delta of the Ganges appears the birthplace of cholera; it is, however, readily fostered, under certain hitherto inscrutable conditions, by the aggregation of men in large numbers, especially when fatigue, bad food, bad air, and contaminated water, excitement and subsequent exhaustion, and other deficient sanitary conditions, continue to depress those collected together.

As yet we are unable to say why cholera should prevail at one time or season more than another; though the commencement of epidemics apparently have had reference not unfrequently to failures, or damaged states, of the rice crop in India.

Without pretending to decide as to the amount of importance of any one, we would enumerate the following as conditions on which the spread of cholera has appeared to depend.

An unexplained condition of atmosphere and season; human intercourse, and contagion to some extent, favouring conditions of locality or dwellings, such as tend to render the inhabitants also prone to fever and other diseases. A debilitated state of health, as from intemperance, excesses, over-fatigue, exposure to great alternations of temperature, wettings and chills, &c. Cachexy from disease, if not predisposing to cholera, undoubtedly lessens the probability of recovery. The reception into the system of matter (either by the lungs or with the aliment) excreted from persons in cholera, or suffering from some forms of diarrhœa.

Attention to the following conditions has been attended with evident benefit in many climates.

Good sanitary conditions of locality, dwellings, ships, &c.; good food, sufficient occupation; and avoidance as much as possible of the conditions enumerated above, as injurious at times of epidemics. Observance of the utmost cleanliness in and about those sick of diarrhœa and of cholera, and thorough purification of all dwellings, ships, clothing, furniture, &c., used by the sick, or by persons that had sojourned in infected localities. The destruction by fire or burial of all discharges from the sick, and of garments or bedding, or other things not admitting of thorough cleansing, or of exposure to a temperature of 250°. Avoidance of contamination of all water, or soil near water, or near dwellings, by discharges from the sick, or by the washings of infected garments, &c.

Many writers have thrown much light on the actual state of the body during cholera, founded on the symptoms during life, and on post-mortem appearances.

It would exceed the limits allotted to us to attempt even to enumerate the solutions that have been deduced by these observers from their researches. We can only shortly state what we think best explains the symptoms during life, and the post-mortem appearances, as well as accounts for the convalescence and recovery of persons affected with the disease, and the action of agents employed as remedies.

Some have attributed much of the disease to the state of the blood, many to that of the nervous system; others, again, too numerous to name, have regarded it specially as a lesion of the alimentary functions, and others consider the respiratory apparatus as chiefly in fault; and, again, the circulation has been regarded by some as specially arrested, while others have pronounced cholera as a form of fever.

That, however, the symptoms and course of the disease depend upon a poisoning of the system is a position held by many, and that its phenomena may be thus explained also appears reasonable. There is nothing in medicine to negative the fact that a poison may enter the system by the lungs and by the great alimentary tract, for we see that a vitiated atmosphere poisons through the lungs just as impure water has been known to produce ague; diarrhœa and dysentery may also owe their causation to either mode of admission to the system; always provided that other conditions, such as sufficient length of exposure, relaxation, fatigue, &c., have co-operated.

Now, if to these means of conveyance into the system we add another, namely, a certain state of the air, whether as to electricity and oxygen or ozone (if we are still permitted the term), we will not stop to discuss; then we have the concurrence of three of the elements on which our life so much depends; and if to them we add the co-operation of temperature, we see how efficient for good or evil their joint action may be.

Further, when we reflect how powerfully all organized matter, whether solid or fluid, is acted on by these, we must be convinced that our bodies depend for healthy life on the concurrence of a favorable state of these elementary agents.

If we literally accept the interpretation afforded us by Holy Writ of the nature of the blood, we can at once see how readily its deterioration may be effected by the formidable combination of elements above named.[1]

If, then, the blood, that which we for the moment claim to be the life of the body, be deteriorated, we can easily understand the alarming train of symptoms thence arising.

It will be asked on what other grounds we base our reasoning; we answer, on the results of actual research, many have ascertained that the state of the blood is altered in cholera; we do not claim for it any other constant state, but that of departure from the healthy standard, and of this we have much evidence—that of ordinary observation, as well as that afforded b the microscope and by chemical analysis; the frequency o cutaneous affections indicative of blood disorder, &c.

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[1] Also Haller, Hunter, Hewson, &c.

Again, a mere draining off of the fluid portions of the bloo will not account for the facts of cholera; we repeat it—nothin but an altered condition of its vitality, nothing but somethin that profoundly alters the nerve-force, the co-ordinating power of animal life, will explain the symptoms of the disease, the nutrition and action of the nerves being dependent on the blood.

The valuable and admirably systematised labours of Baly an Gull, the records afforded by our French brethren in the epidemic of 1865-66, which, with the contributions of German and continental Europe generally, of America, and of Britis physicians from almost every region of the earth—all tend t prove that cholera does not consist in this or that symptom, o in any one condition of the blood-vessels, or of the alimentar canal, or even of the nervous system, but in some great defec of the whole economy. Now, loss of vitality of the blood[1] mus evidently be regarded in this light.

We would here quote an authority to whom we are much indebted for the light he has thrown on the nature of cholera b his laborious investigations in India. Dr. Parkes observes—

> "When we remember the great share taken by the blood-globul in the respiratory and heat-furnishing processes, it is scarcely possible to avoid concluding that their loss of salts is connected wit the characteristic cyanosis and lowered temperature in cholera. I most cases there is vomiting and purging before there is loss of hea though this very soon follows in a slight degree, and then graduall augments. In other words, the diarrhœa coincides with the firs chemical changes in the blood—the transudation of some of the constituents of the serum. The lowered temperature follows afterward at the time when we know that diffusion from the blood-cells int the serum must be taking place, and augments gradually as the di fusion increases."

Dr. Aitken, from whose recent edition of the 'Science an Practice of Medicine,' &c., we have transcribed the foregoing observes—

> "In all the [illegible] examined by [illegible] Marcus at Moscow, in 1832

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[1] [illegible] ...ine m[illegible] ...t, others that phosphorus i [illegible] ...litions being altered, dialysi [illegible] ...hose of the intestines, occur

the clot and serum evinced acid qualities on the application of litmus, except in four cases, where the discharges were watery and the reaction alkaline. The phenomena of the disease may thus be traced from the transudation of serum constituents as the starting-point. All the other chemical changes in the blood and the most marked symptoms, such as the abnormal respiratory process, follows as a matter of course.

"Such (continues Dr. Aitken) is the nature of cholera, according to the observation of Drs. Parkes, William Robertson and Schmidt, and thus an early theory of the nature of this disease has received the support of one of the best chemists of the day, namely, that the blood is the primary seat of the disease, and becomes contaminated by the absorption of a specific poison."

But we have additional evidence to that afforded by our senses by chemistry, the microscope, by morbid anatomy, and by inductive reasoning upon the symptoms of the disease. We have, too, the corroborative testimony of suitable prophylaxis and treatment.

Again, the very lesions of function, and those left after the fatal supervention of consecutive disease, all tend to establish the correctness of the idea that the blood is the seat of the lesion, and that it is to its deterioration and even death that we must refer the fatality of cholera.

That it is, in fact, a disease or even death of the vegetative life has been shown by able French observers. Now, whatever the relations between the life of the body and that of the soul may be, we know that the separation between the two does not take place with apparent death from some causes; thus, drowned persons and others may be resuscitated. Let us see how far we can reconcile these facts with those of cholera, on the assumption that the latter is dependent on a diminished vitality of the blood. As in drowning, so in cholera, those cases apparently beyond hope have been restored by energetic and judicious efforts, continued sufficiently long.

Let us admit that the constant morbid condition of cholera resides essentially in the blood, then we can understand how much locality, food, water, and dwellings will render persons more liable to the disease; and if we also admit that somehow or other the blood represents the principle of life, and that according to the researches of Parkes, Garrod, Grainger, Field, Papillon, Schmidt, Robertson, Marcus, and others, the blood is vitiated, we must be guided in our treatment accordingly.

As the morbid changes in the blood are in many cases a gradual process—and as in a large per-centage, though not in cases of cholera, these changes are evidenced by diarrhœa, again is usually classed as the premonitory stage of cholera—the major part of our profession at home and abroad and the records of the epidemic of 1854 sanction the attempt to check this symptom by "chalk with opium, ammonia and catechu, or by sulphuric acid, with or without opium, and with calomel as an adjunctive remedy," &c.

It will have been seen, however, in our preceding pages, what the treatment has been in France, and that several persons, reasoning upon post-mortem evidence, and filling in the links of that evidence, where deficient, from settled ideas of their own, and forgetting, as a valued contemporary points out, the ever-to-be-remembered saying of Hippocrates, "Curationes morbum ostendunt," insist that purging is to be encouraged. As well might they say purging should be encouraged in the many forms of fever in which diarrhœa forms frequently a harassing symptom.

To such persons we would say, "Remember the anatomical lesions so often found in the intestinal tract in cholera. Would you use purgatives in peritonitis?" And yet the autopsy not infrequently shows no small analogy between the two disorders. It is true in the one the inner lining of the intestinal tract is most usually the affected part. Did time and space permit us, we could enumerate many facts, and show many analogies to prove the extreme danger of views put forward with great perseverance.

Granting, then, that the alimentary tract chiefly is that by which the system shows the presence of a poison, and to a certain extent allowing that alterative action is proper in many cases, we have confidence in recommending ipecacuanha, with or without small doses of opium. We have long been in the habit of using ipecacuanha in some stages of dysentery and in certain forms of hæmorrhage, partly from having learned its use in different climates, as recommended by many, and partly from our own observations. We are now speaking of diarrhœa and choleraic diarrhœa, and we only use it occasionally, trusting to quieting the system by rest in the recumbent posture, with chalk mixture, with or without opium, in small doses, and no sugar, &c., and combating internal congestion by the ipecacuanha and frictions, and external applications, &c., to the abdomen.

We have only used ipecacuanha in small doses, from one to four or five grains in the adult; and we would remark that the use we make of opium is not unlike that expected from it in Dover's powder and in the ordinary oil draught in diarrhœa, caused by irritating ingesta, because while we cannot set too high a value on its effect in soothing the system and preventing spasm, we well know its dangerous effects in tending to coma when given by routine in cholera as in fevers of certain types. Mercury, especially as calomel or gray powder, with or without ipecacuanha or opium, is also in suitable cases required, and is, when used with caution, of the highest value. Sulphuric acid, properly diluted, very often (alone or in combination with light bitters) has a valuable alterative antiseptic and astringent effect.

Diluents, as chicken broth, water, pure or acidulated, are useful, as well as ice, under some conditions. Stimulants, as ammonia, brandy, pure wines (still or sparkling), ethers, &c., with external frictions, often rouse the system, and enable it to overcome the deadening effects of the disease. Carminatives are also often useful.

As the disease progresses into established cholera we use the same class of remedies, omitting opium (*wholly* as a general rule), and calomel, unless under circumstances demanding its use. We have certainly seen both calomel and opium, given even in small doses, produce, if long continued, most injurious consequences. It is only right here to say that both in France and in India emetics are recommended on respectable authority. Mustard and ipecacuanha have both thus proved useful. We have no personal experience of them. Stimulants too freely given are highly dangerous. Enemata of warm liquids, and as a means of nutrition or of stimulation, have proved useful. Above all things avoidance of hot baths and vapour baths and wet sheets, and almost as absolutely of taking blood, should be insisted on. Although we think, as a general rule, it is essential to abstain from taking blood in cholera, there are cases in which even this may be admissible in the early stages of the disease, as mentioned by many of the older writers on the subject. One case occurred to ourselves in which the symptoms of cholera (with total suppression of urine) in a plethoric and powerful young man seemed to call for such a step, and most marked relief, indeed we may say cure, appeared immediately to follow venesection to twelve ounces. We may mention this case with the injunction that in the vast majority of cases venesection has proved highly injurious.

Frequent supervision by judicious medical men is essentially requisite, and for this purpose relays of practitioners should be invariably appointed to all institutions for the treatment of this most formidable malady. House-to-house visitation should be conducted in the same way. In collapse we should remember that many apparently dead have, by perseverance with judicious measures,[1] been rescued from the very jaws of death.

We confess that the daily increasing amount of literature on this at present most important subject has rendered our task a difficult one, as to attempt to embrace the views of all the writers would simply be impossible, even within far wider bounds than ours. We are, consequently, reluctantly obliged to omit many notes we had made when the amount of matter was of much less extent than it is at this the last moment at our disposal. There are several points corroborative of the views we have advanced above which we have been obliged to pass over; one which occurs at this moment to our memory—the similitude between some of the contents of the bowels and of the evacuations and the exudations of diphtheria—require more than this passing notice. Also the important question of the so-called eliminative treatment would have merited a fuller consideration than it was possible here to give it. We would, however, remind our readers again of the necessity of not receiving any theory, though supported by unquestionable facts (as the theory in question is not), in the face of accumulated experience.

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[1] As frictions with stimulants, bags of hot sand, bottles of hot water, mustard poultices, galvanism, combined with bland light liquid nourishment in small quantity often given, water, warm wine, &c. &c.

We make no apology for giving, though necessarily briefly, an abstract of actual practice in the recent and preceding epidemics, convinced of the great value of such records, though perhaps not in strict accordance with what is conventional in such a periodical as ours, and to these records we would specially direct attention: in them, too, will be found notice of the treatment of the sequelæ of the disease, a subject that space does not allow our entering upon at present at any length. The necessity of treating the cerebral symptoms with some energy will be seen in the records of practice (as just alluded to) from the Saint Antoine Hospital, in Paris. A case that came under our observation of fatal disease consecutive of cholera that presented cerebral symptoms, coupled with obstinate purging of greenish stools, with the admixture of whitish shreddy particles, we attributed to the large doses (ten grains) of calomel that had been repeatedly given in the earlier stage of the disease. Looking through different authors on this subject we find Dr. Goodeve draws attention to it thus:

> "The purpuric conditions, besides the causes already mentioned, may be produced by large doses of calomel. When this was used in scruple doses to check vomiting it was not uncommon to give two or three such doses.
>
> "One can imagine that forty or fifty, or even fewer grains of calomel, accumulated in the stomach and intestines might be absorbed in reaction, and be ready to add its liquefactive action to the other blood-dissolving agents already at work. Cases of this sort which I have seen have made me cautious about the use of large doses of mercury in cholera."

Blisters to the loins have been recommended by some to relieve the suppression of urine. A. P. Barrant, M.D., of Mauritius, strongly advised quarter-grain doses of extract of belladonna and a mixture of water and white of egg, *ad libitum*, as a diluent; the latter, as already suggested in a former number of this Journal, probably acting usefully in the relieving cardiac abrasion that often coexists in cholera with hiccough, as well as relieving thirst. Those who are of opinion that cholera depends on arterial spasm will probably bear M. Barrant's treatment in mind. They will, however, do well to remember the power, for evil as well as for good, of belladonna when using it.

We draw an unfavorable inference from the fact that though Italy has suffered much of late from cholera, we have heard nothing of the use of the sulphites in the disease, as we hoped Professor Polli's ideas would have been put to the test on this subject.

We have now, however imperfectly, discharged our task, one which truly merited more space than we could devote to it, as the subject of cholera is one not only of the deepest moment to all communities, but also one of paramount interest to the inquiring physician as well as to the physiologist.

To the philanthropist, too, as well as to statesmen, it is of the highest importance. Some hope has been given by recent events to the idea that after all cholera is to a great extent a preventible disease. We sincerely trust that this may prove correct. It is also satisfactory to think that measures calculated by experience to lessen the probability of its visiting a locality, are also likely to prevent the origin and spread of fever.

PART SECOND.

Bibliographical Record.

ART. I.—1. *Philosophia Zoologica.* Auctore J. VAN DER HOEVEN. *Lugduni Batavorum,* apud E. J. Brill, 1864.

Philosophy of Zoology. By J. VAN DER HOEVEN. *Leyden:* E. J. Brill, 1864.

2. *Catalogus Craniorum Diversarum Gentium,* quæ collegit J. VAN DER HOEVEN. *Lugduni Batavorum.* E. J. Brill, 1860. *A Catalogue of the Crania of Various Races,* collected by J. VAN DER HOEVEN. *Leyden:* E. J. Brill, 1860.

3. *De Vera Anatomes Comparatæ Indole. Oratiuncula ad aperiendas Scholas, quam habuit die* xxvi *Septembris.* J. VAN DER HOEVEN. *Lugduni Batavorum et Amstilodami,* apud J. H. Gebhard et Socios, 1848.

On the True Bearings of Comparative Anatomy. An Inaugural Address delivered at the Opening of the Academical Session on the 26th of September, by J. VAN DER HOEVEN. *Leyden and Amsterdam:* Gebhard and Co., 1848.

To the ordinary Englishman it is not a little vexatious to see advertised a work by some such Dane, Swede, or Dutchman as Eschricht, Retzius, or Van der Hoeven, and to recollect that it will remain a sealed book to him till some kindly Dr. Moore or Professor Clark step in and translate it for him. In such times of tantalising prospect, the fifteen years spent in mastering more, or, as we fear, often less perfectly the tongues of ancient Greece and Rome may seem to have been a somewhat prodigal allowance to be assigned out of the sixty years in which man can labour; and it is with a pleasure the more real because now so rare, that we come upon a modern scientific treatise, written in the same language as the Epigrams of Martial, and the Ars Amandi of Ovid. Three works by Professor Van der Hoeven already well known to us by the Cambridge translation of his "Handbook of Zoology"—written in Latin, and suitable therefore to the Englishman of sound classical education, and bearing the titles of 'Philosophia Zoologica,' of 'De Vera Anatomes Comparatæ Indole,' and of 'Catalogus Craniorum Diversarum Gentium,' we propose to introduce herewith to the notice of our readers.

The 'Philosophia Zoologica' carries us back, by the association of title, nearly sixty years, to the days when a 'Philosophie Zoologique' appeared at Paris, *auctore* J. B. O. A. Lamacrk; and we venture to suggest that the Professor of Leyden, in 1866, intended by the name he chose for his work, to remind us of this now all but wholly forgotten fact in the scientific history of 1809. Be this as it may, the appearance of this work is most opportune in this country at the present juncture when Dr. Carpenter's large work on Comparative Physiology is out of print, and when M. Milne-Edwards' 'Leçons' have just filled up a ninth octavo volume. Professor Huxley's excellent 'Lectures' are professedly and confessedly but one volume of a continuously, as we hope, forthcoming series, and, whilst we are waiting for his advertised 'Manual,' the admirable existing German Handbooks are about as available for the uses of ninety out of every hundred of the ordinary Englishmen already alluded to, as would be the Targum or Zendavesta.

The Latin manual of the Dutch anatomist and zoologist, makes up a moderate-sized octavo of some 400 pages, 190 of which are devoted to anatomy, under the several denominations of general, histological, comparative, and osteological; 80 to general and comparative embryology; 25 to the purely zoological subjects of nomenclature, taxonomy, and description; and 84, concluding and most interesting pages, to the geographical distribution of animals. These are large subjects to be treated of within such narrow limits, and it is only a master-hand which can deal with such subjects becomingly and worthily under such conditions. Such a hand is Professor Van der Hoeven's. Still we find, that with conscientious punctuality he gives the fullest references in his notes to the best authorities, on each of the very many matters which he expounds in his clear and easy Latin text; and the advantages of such bibliographical details we need not dwell upon, to our readers who are familiarised to the use of similar accessories by Dr. Murchison's laudable practice in his 'Treatise on Continued Fevers.'

It is somewhat surprising to find so little mention made of the great question as to the "Origin of Species" in a work with the title of 'Philosophia Zoologica;' and anxious though we were to possess ourselves of Professor Van der Hoeven's views as to Mr. Darwin's doctrines, we had some difficulty in finding a statement of them. But at page 275, where man's power to render permanent, by domestication and its influences, such varieties as may arise amongst his flocks and herds, and may give promise of being more than ordinarily profitable to him, is discussed, we find a very sufficiently unambiguous hint as to the opinions of our Dutch philosopher. From his words, which we will forthwith proceed to quote, it is plain that Professor Van der Hoeven has resisted the fascination to which his contemporary Schleiden, as well as so many other and younger naturalists have yielded. His words are these—

> " Œconomi seligunt in domesticis animalibus eas, quas ob varios usus aestimant plurimum, et seligendo constantiores faciunt atque emendant. Simile quid naturæ opus esse vix idoneis argumentis probatur: natas esse hac ratione species et etiam nunc novas nasci temere affirmant recentiores non nulli. Ne unum quidem exemplum adferri potest speciei hac ratione ortae. Species longissimo temporis intervallo mutari non nulli affirmant, nostram vero experientiam augustis nimis limitibus circumscriptam esse volunt ut has mutationes videamus. Quid ingens temporis diuturnitas efficere possit, profecto ignoramus, sed id tamen intelligimus breviori intervallo aliquam mutationem fieri debere necessario, quæ sit observatione comprobata, ut exinde majorem, quam volunt illi, explicare possimus. E nihilo, productione temporis vel millies multiplicato, tamen nihil fit. Cf. Cuvier Rech. sur les os. foss. I. Discours sur les révolutions de la surface du Globe, pp. 62, 63. Scite Decandolle hanc opinionem, quæ mutabiles esse species sumitur, et improbabilem et sterilem dixit (Theoria improbable . . et inutile, puisque, si elle était vraie, nous devrions, sous peine de ne rien savoir, nous conduire comme si elle était fausse, et étudier, comme aujourd'hui, les formes les plus habituelles des êtres." ('Théorie élémentaire de la Botanique.' Paris, 1819, p. 196.)

Such, it seems, is the kindliness of Professor Van der Hoeven that he cannot bring himself to pronounce a sentence of strong condemnation with his own lips, and he calls in a French philosopher to perform the painful duty for him. It is noteworthy that he does not avail himself of the somewhat undignified expression of the same opinion which M. Fleurens' works might have furnished him with, albeit he quotes this author on the page immediately preceding. We make bold, however, to suggest to the veteran of Leyden that in the next edition of his admirable 'Philosophia' he make mention of the views put forth on this matter by Schleiden, and by V. Baer in the conclusion to his 'Memoir on the Papuans and Alfoutons' in the 'Memoirs of the St. Petersburgh Academy for 1859.' He will recognise in either a worthy compeer; and of the latter he speaks not more gracefully than truly as " Vir de omni disciplina Physiologia meritissimus," at page 135 of this Latin book; and has written a memoir of him within the present year, we doubt not in equally laudatory, but in, to us, unintelligible Dutch, in the 'Nederland-Tijdschrift voor Geneeskunde.' We must here, and now, content ourselves with observing that the Darwinian reading of that "mystery of mysteries," the origin of species, professes to base itself upon two other main supports, viz., the phenomena of the distribution of species and the existence of rudimentary structures, which are in great measure or altogether independent of the one the validity of which is here impugned. And for the final and summary setting aside of any conclusion, it is necessary to show the inadequacy not merely of one, but of all the lines of evidence upon which it is based.

The chapters on the distribution of species contain a vast amount of most interesting information, collected with an amazing industry from the works of a series of writers, reaching from the days of Buffon down to those of our own distinguished zoologist, Dr. Sclater. Very much of the matter contained in these 84 pages will be as novel as valuable to the English reader. The subject is treated of under two heads, viz., that of Geographical Zoology, which gives a history of the way in which the great classes of the animal world are spread over the globe and of the instances of strict limitation to narrow areæ; and, secondly, that of Zoological Geography, which, as it shows us that the whole of our globe may be mapped out into areæ occupied by complex assemblages—armies, in fact, of all arms, of mutually dependent creatures—may be spoken of, in a word, as being Geography taught by hieroglyphics. Where there is so much to praise, it is doubly ungracious to express dissent; still, we must say that we are surprised to find Professor Van der Hoeven lending at pp. 321, 322 his sanction to the doctrine of the plurality of centres of creation, a doctrine which his words at p. 27 of the Introduction to his 'Handbook of Zoology' (English translation) would appear to disclaim. Now, Mr. Mill has taught us not to place too implicit a reliance on the "Law of Parsimony," albeit Newton placed it first among his "Regulæ Philosophandi," in the beginning of the third book of the 'Principia;' but that maxim has yet a legitimate place and power, and the naturalists of the Old World have, we think, done well in following it by disallowing, as most of them have done, in contradistinction to their American compeers, a plurality of areæ for the origination of single species. If different, distinct, and distant spots on our earth's surface had witnessed the coming into being for the first time of a single species in the persons or organisms of different and distinct representations of such single species, surely we should expect to find, or even, let us say, we should find that similar *stations*—such, for example, as equatorial islands—would be inhabited by similar and indeed identical animals. The Cave Fauna of Kentucky would be identical with the Cave Fauna of Carniola; degrees of latitude would count for more than continuity of area; and, in one word, *station* would be but a synonym for *habitation*. Nature, however, has arranged matters otherwise in fact.

In the second part of the chapters on this subject, that which shows us how the whole of *terra firma* is divisible into seven distinct areæ, each alike occupied by a complex army made up of all animal denominations, we think sufficient prominence is scarcely given to the great principle according to which the spreading of animal, and indeed to a considerable though less extent, of vegetable organisms is more immediately dependent on the relations they hold to other and—those sometimes rival and sometimes not—rival organisms than on any purely physical conditions of heat or cold, latitude or longitude. As no man can live and as no man can die to himself alone, so in the world of brutes no one animal lives or dies without affecting or being affected by the well-being of its fellows in creation. The present distribution alike of plants and animals depends primarily on their "centre of creation"—on the spot, that is, which witnessed their first origination, and on the connection which it has, or in geological times had, with other areæ. But of all secondary and now acting causes, mutual inter-dependence is the most potent; and it outweighs, even in the case of vegetables, and more markedly much in the case of animals, the merely physical conditions of cold and heat, dryness and moisture. The grasses of the Cape of Good Hope are dependent for their carriage on the antelopes which feed upon them; the red clover near our towns on the cats which eat the humble bee-destroying field-mouse; the life of the newly-dropped ruminant depends on the parasitic insect's success against the fly, which would else lay eggs in its umbilical scab. "And so on in ever-increasing circles of complexity."

It is, however, an insidious task to point out defects, and, like the flies we have just spoken of, to fasten on the single assailable point in a great structure. As it is, we can but find space for a special recommendation of the 100 pages on Comparative Anatomy, and the 80 on General and Comparative Embryology; and we conclude our notice of the book with a general and strong recommendation of it to all who are interested in any part of the wide territory over which Professor Van der Hoeven ranges.

The two other works, the titles of which stand at the head of his article, possess one fault—they make up together not quite

100 pages. They are addressed specially, the one to ethnologists, the other to comparative anatomists; and to persons deserving of these honorable titles it would be superfluous to recommend them in detail.

ART. II.—*Sulla Causa specifica del Colera Asiatico,*[*] *il suo processo patologico e la indicazione curativa che ne resulta.* Memoria del Dott. FILIPPO PACINI.

A Treatise on the specific Cause of Cholera, its Pathology and Cure. By Dr. FILIPPO PACINI, Professor of Microscopical and Topographical Anatomy to the Medical School of the Institute at Florence, &c. Pp. 62.

THE present treatise adopts the view that cholera is attributable to the destructive action of parasites; the development of this opinion is aided by physiological interpretations, which have not yet found their way into print in an extended form, but of which we are now permitted such partial glimpses as seem necessary to the comprehension of the subject. They teach that the veins are the chief instruments of absorption, and that the so-called absorbent vessels are merely adjuvant and complementary, opening by free or expanded orifices into interstitial spaces or lacunæ in the web of the tissues, in similar manner to the veins of the invertebratæ. In fact, according to the author, lymphatic absorption is a myth, for it is merely by the circulatory impulse that the lymphatics are filled with any superfluity not absorbed by the veins. Dr. Pacini has observed that the operation of the parasites in cholera is primarily on the epithelium and villi, eroding the mucous membrane and laying bare the capillaries. In the general run of cases the epithelium and villi which have been subjected to a destructive process are thrown off during the premonitory diarrhœa, and are discoverable with a little difficulty even in the earliest dejections and vomited fluid, after which only plentiful shreds of mucus are to be met with in the stools; it is otherwise, however, in cases of extraordinary rapidity and urgency, in these the more abundant epithelium and villi are found without difficulty. The essential character of the disease is made to lie in the loss of fluid lymph; it is a true lymphorragia, not far different from hæmorrhage. The want of balance ensues between the processes of absorption and secretion: when once the critical border line is passed—when the scale has fallen on the wrong side, there is nothing to be looked for but death, except from the occurrence of that algid condition in which a blood-stasis forbids any further exudation of fluid. If a lapse of time and the vital powers now allow of repair in the destroyed tissue, endosmosis will bring assistance to the vessels, and the process of absorption also in all parts of the frame will tend to restore the activity of the circulation; but in cases in which death rapidly ensues, the body is not the least attenuated by absorption, and the same fact is the cause of astonishment in cases of prompt recovery.

Owing to the extent of surface implicated by these parasites in the intestinal canal preventing absorption of fluid ingesta as well as repair of waste by food, the thirst in cholera is intense and unappeasable. Their destructive action is comparable to that of perforating ulcer, allowing for the minuteness of their abundant and incalculable erosions; the fact of the existence of parasites is based on Dr. Pacini's microscopical observations, and is favoured by those of certain German pathologists quoted by Gull in the 'Report on Epidemic Cholera,' p. 15, London, 1854; also by experimental inquiries on the atmosphere surrounding the choleraic, by Dr. Thompson, in the same report. Virchow also has drawn attention to certain diphtheritic exudations.

The treatise of Dr. Pacini is well written and keenly argued; on this account alone it is worthy of a perusal, apart from the originality of his observations. As regards treatment, it falls in with the view that astringent and antiseptic medicines constitute our main resource in cholera, accordingly, phenic acid or kreasote seems best to combine these qualities. Frictions, heat, or any direct interference with the algid condition, would be considered as opposed to the best chances of cure.

ART. III.—*On the Rapidity of Thought and of the Determination of the Will:* A Preliminary Communication by Dr. F. C. DONDERS. Translated from the *Nederlandsch Archief voor Genees en Natuurkunde,* 1e Deel, 4 Aflevering, 1865. Pp. 518. By W. D. MOORE, M.D. Dub., M.R.I.A.

WITH the co-operation of Heer de Jaager and of some other pupils of the University of Utrecht, I have made investigations as to the rapidity of conduction in the nerves, and, more particularly, upon the time required for the formation of a definite idea, and for the expression thereof through the organs of the will.

The physiological time T is the time which elapses: from the acting of a stimulus upon the peripheral nervous system to the giving of some signal.

1. T is greater on irritation applied to the foot than on irritation in the groin: the difference is the time required for con-

duction in the nerve from the foot to the groin. For different persons an average rapidity of conduction of 26 and 26·09 in 1″ was thence deduced.—In the average of experiments of one series the probable error was very slight; for that of different series, particularly on different days, it was comparatively great.

The method used in these experiments consisted in giving an induction shock, with opening of the current, and subsequent closing of the same by pressure with the hand. The time during which the current remained open was registered by an electro-magnet on a revolving cylinder. The results differ little from those of Hirsch and Schelske.

2. Closing with the right or left hand takes place nearly in the same time; there was a difference of 0·009″ in favour of the right hand.

3. T is greater when, without the subject of the experiment expecting it, the right and left groin are alternately irritated, and the closing has to be effected respectively with the left and right hand. The difference amounts to 0·066″.—In this experiment two keys, *r* and *l*, were used, so arranged that on pressure on *r* the current was closed, if the right was stimulated; on pressure on *l*, if the left, while on pressure on both at the same time no closing took place.

4. The reaction to the stimulus of light was investigated by means of an apparatus, such that at the moment of opening the current a bright light fell upon the eye, and thereupon, just as in 1, the current was closed, and the time it had remained open was registered. T was now compared for the cases in which the signal followed simply upon the impression of light, and for those where on the appearance of red light the right hand, on the appearance of white light the left hand, had to press the key. The time required to arrive at a definite idea and for consideration in the choice of the hand was found to be in different observers: 0·159″, 0·122″, 0·134″, 0,184″.

In this case it was observed, that one is strongly inclined to picture to one's self what colour shall appear, and that, when the colour answers to the expectation, less time is required; more, on the other hand, when the expectation is at fault. A special investigation on this point is made.

5. In order to determine T in impressions of sound, the phonautograph was used as a chronoscope. Two persons, A and B, lie with their heads sufficiently separated by a partition, in the phonautograph. While the cylinder is turned and a tuning fork of 261 vibrations in 1″ registers the time, A utters a strong vocal sound, for example, as *ki*. As quickly as possible B calls out the same. The commencement of both sounds

is accurately marked upon the cylinder. T is read off from the number of corresponding vibrations between the commencement of A and that of B.—These experiments were alternated with others, in which A first cries out *koo, ko, ka, ke, ki, kö* or *kü*, and B must repeat the same sound. The difference in the two cases gives the time required for the idea of a definite sound and the reaction in connection with that idea. The results were:

A. DONDERS.		B. DE JAAGER.	
	Average in vibrations of $\frac{1}{261}'''$		
Indefinite sound, T′ =	99·05	86·73	79·33
Definite sound *Ki*, T =	74·36	62·68	59·15
Difference	24·69	24·05	20·18
A. DE JAAGER.		B. DONDERS.	
Indefinite	69·82	69·82	69·82
Definite	46.44	49·64	45·
	23·38	20·18	24·82

On the average of all the observations the difference found is 22·88 vibrations of $\frac{1}{261}''$ = 0·08767 seconds.

6. In order to find the time required merely to form an idea, Donders tried with what difference in time between an impression on the eye and on the ear it is possible to distinguish which of the two impressions preceded the other.

ART. IV.—*Some Effects of the Climate of Italy.* By THOMAS KING CHAMBERS, M.D., Fellow of the Royal College of Physicians, &c. *London*, 1865, pp. 95.

IT is one of the characteristics of ability to adorn whatever it touches—*nihil tetigit quod non ornavit.* Such was the impression left on our mind after reading this charming little book.

Its author tells us, by way of introduction, how, owing to failing health after a severe operation, he found himself unequal to his ordinary duties; how he was advised by his professional friends to discontinue them for a time; and how, to be free from the temptation which the lover of work so well knows it is difficult to resist, he resolved to seek rest in travel; and further, and best of all, how his compliance, after a sojourn in Italy, was rewarded by a complete restoration of health and working power. His own failing health, without any special malady, a state analogous to what is called, in common parlance, a break-up, or broken health, or half death, as he emphatically designates it, becomes the occasion of his considering and describing this obscure morbid condition. His account of it is such as might be expected from a physician of his experience, and is excellent.

He divides it into three stages. 1st. Its incipient; marked chiefly by negative symptoms, by functional languor, and especially by a feeble circulation, favouring organic degeneration. 2nd. Its more advanced stage; with distinct disease of some spot, to such a degree as to admit of diagnosis of the organ affected. 3rd. Its most advanced; with severe local developments, such as dropsy, paralysis, consumption.

Of the first stage he is of opinion that a cure may reasonably be aimed at; of the second, he restricts his hope to the keeping it in check, and preventing its getting worse; of the last, he has not even this hope, merely that of somewhat retarding the fatal event, and soothing progress to the grave.

For the detail of the obscure symptoms we must refer our readers to our author's pages, in which they are given with a minuteness and, so far as their vagueness permits, a distinctness which is hardly mistakeable, and which to the student cannot but prove very informing.

Now, it is this condition of broken health, such as he experienced in himself, that in a remediable point of view he considers the climate of Italy peculiarly favorable. He, of course, does not rest his belief on his own case, but on the largest induction of facts which he could avail himself of, all tending to prove, comparing Italy with our own country, that in the former acute diseases preponderate, in the latter chronic; and that the excess in the latter "is most striking in diseases where a tendency to degeneration of tissue is most marked and most distinctly the cause of disease."

The Italian climate, under the heading of "Powers of the Air," has next his attention. We admire the manner in which this part of his subject is treated, and the felicitous style used in the description of the climatic influences to which the invalid is most indebted for a cure or relief. Amongst those influences stand foremost a clearer sky than our own, a warmer sun, a drier air, a more equable temperature, and these chiefly in connection with a sea, the Mediterranean, warmer and somewhat salter than the Atlantic, and the Alps and Apennines, those great natural screens sheltering the peninsula in its northern aspect.

These climatic influences he holds to be beneficial in chronic ailments with defective power and a languid circulation conducive to degeneration, by stimulating and so producing healthy action; and, on the contrary, injurious in acute diseases, in which excitement is contra-indicated, and especially in nervous complaints, which, from his inquiries, appear to be more prevalent there than in our colder and damper atmosphere. So far as we can judge, we can see no flaw in this part of his argument, and we quite agree with him, that the beautiful picture which the scenery of Italy presents, and the vast variety of objects of interest of human art of various ages which there so frequently meet the eye, may be a curative help, by engaging the attention and amusing the mind of the intelligent traveller. One passage particularly pleased us, in which he deprecates loitering idleness and lauds useful labour. He says—

> "*Experto crede*, holidays are no holidays after a few weeks, or at most months, are over; and earthly happiness is to be found only in steady regular work. There cannot be a more dreary purgatory than the purposeless existence led by some of our countrymen resident without fixed occupation in foreign parts, either tiring themselves out by flitting, with foolish perseverance, in search of amusements which have ceased to amuse, or gaping with open shells in a vain hope that the tide of events may wash some food into their minds. One almost pardons them their scandal-mongering and still worse modes of excitement, when it is remembered that this is all that distinguishes their lives from those of the butterfly or oyster. I shrink from exposing any one to this trial, except as a matter of stern necessity; and after what I have seen abroad it must be a stern necessity indeed which makes me counsel a patient to throw up for ever home duties, and live and die in a strange land."

We offer this quotation partly for its truthfulness and partly as an example of his animated and picturesque style. Southey, the indefatigable, we may remark, had the same view as to work expressed in his motto, *In labore quies.* Should not this afford comfort to some of our complaining, hard-worked brethren?

In the latter pages of the book the places most eligible for invalids are pointed out, and reasons are assigned for their preference. Those he most approves for persons labouring under the broken health in question are such as enjoy, in highest degree, the qualities of climate already mentioned. For a fixed home, he specially eulogises the Gulf of Spezia, not the close and crowded town, but a villa in its neighbourhood, with grounds sloping to the shore; and in Sicily, for a winter residence, Palermo. As regards localities, his book can be consulted with advantage, especially as relates to the principles which should guide the selection. According to our own experience, the vegetation of a spot is one of the best criterions of its climate; and that *cæteris paribus* as to soil, wherever the orange, the lemon, the olive, are found to flourish and fruit, there you may be sure the winters are mild, and frost and snow are almost unknown. Travelling from France into Italy by the beautiful coast line of road, a striking proof of this is afforded. Until you pass Avignon, the olive is a stunted shrub; not until you reach Montoni and St. Remo does it attain the dignity of a forest tree, rivalling in size that of the Ionian Islands.

The author, after offering suggestions about diet, concludes with laying down a certain number of rules for the invalid, which, limited as we are for space, we need not dwell on; but we must not omit mention of the last advice that he offers, viz., that Italy is not the place for a poor man, and that none should cross the Alps in search of health without the means of commanding the comforts they have been used to have. Whether he is right in his qualification unless they can rough it, we are somewhat doubtful.

ART. V.—*Observations on Meat (butchers' meat) in relation to the changes to which it is liable, under different circumstances.* By JOHN DAVY, M.D., F.R.S., &c.

THIS communication originally appeared in the 'Proceedings of the Royal Society of Edinburgh,'[1] and contains the results of careful experiments on change in meat modified, (1) by degree of temperature; (2) by a moist or vaporous atmosphere; (3) by cooking; and of observations on the influence of sulphurous acid and acetic acid in arresting putrefaction.

As respects the effect on meat produced by different temperatures, Dr. Davy found that at a temperature of from 60° to 65° Fahr., in August, portions of lamb exposed to the air lost weight rapidly and soon became dry and hard, without acquiring any putrid taint; whilst other parts softened and for the most part liquified, becoming putrid. Over water in vacuô at the same temperature, for the same time, meat was well preserved. In winter a portion of meat weighing 141·1 grains, exposed freely to the air, became dry and hard in twenty-three days, losing 67·7 per cent. by evaporation. Another portion, weighing 74 grains, suspended over water tightly covered for the same time, retained much of its humidity, and shortly became covered with a fine hair-like mildew or filamentous growth; rather more than three weeks later it had lost 65·2 per cent., but its muscular fasciculi were distinct and its structure but little altered; during this time spars from the mildew fell into the water. Dried meat did not attract the flesh-fly, it was only the putrid in progress of deliquescence, when it affords a nidus for the larvæ of this fly and for their nutriment, which did so.

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[1] March, 1866.

At 50° temperature the flesh-fly is not seen, and the deliquescent power ceases and gives way to the mould growth.

Dr. Davy mentions interesting instances showing the effects of dryness of the air in averting putrefaction, even though the temperature be high. This he attributes to rapid desiccation of the surface and retardation of the penetration of oxygen. The quick putrefaction of meat in which the blood remains is attributable to the presence of the oxygen which the blood contains. He points out the futility of burying the carcases of diseased cattle with quicklime, which by its desiccating power prevents decomposition.

Dr. Davy found that boiling or roasting meat or boiling blood arrested putrefaction, and favoured other changes with the production of mould or mildew. Describing the changes undergone by meat and blood exposed to a boiling temperature, Dr. Davy suggests that they may be analogous to that which vegetables experience when converted into peat, a change only taking place at a low temperature—below that favouring decomposition.

Dr. Davy found that sulphurous acid arrested the putrefaction of animal and vegetable substances in an equal degree. Vinegar and diluted acetic acid also acted on them, but did not so alter the character of the substances as to prevent ulterior changes on washing off the acid.

ART. VI.—*On the Existence in the Textures of Animals of a Fluorescent Substance closely resembling Quinine.* By Dr. HENRY BENCE JONES, A.M., F.R.S., &c. Pamphlet, pp. 9.

THE discoveries of science are certainly marvellous! Who, a few years ago, would have been bold enough to imagine that fluorescence, the property of emitting light like fluor-spar when heated, could belong to animal tissues and their fluids under the influence of electrical light; or who could have conjectured that this property is owing to the presence of a substance similar to quinine, so similar, indeed, as to be strictly entitled to the name given it by its discoverers,[1] of quinoïdine! Yet this marvel is shown to be a fact in the most convincing manner by Dr. Bence Jones in the paper before us, which was read at the weekly evening meeting held at the Royal Institution (that prolific mother of great discoveries) on the 23rd of March last, and was at the same time amply illustrated by experiments. Not merely was proof afforded of the similarity of the two by the optical phenomena of fluorescence in the spectrum produced by electrical light, but also by a close similarity of chemical properties.

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[1] Dr. Bence Jones and Dr. A. Dupré.

After stating in a tabular form the proportions in which the fluorescent substance is found in the different parts of guinea-pigs and of man, the author asks—"What, then, is the meaning of this widely diffused substance in animals which so closely resembles quinine?" He replies by the following large and important generalization:

"At present we are far from a perfectly clear answer. It is not thirty years since the presence of ammonia in the products of distillation of coal was considered 'curious,' because nitrogen was thought to be the characteristic of an animal substance, and absence of nitrogen was considered as the distinctive mark of vegetable creation. Gradually, year by year, each substance that has been thought to be the special property of the vegetable world has been found to occur in animals. Thus, sugar, starch, woody fibre, vegetable colouring matter, as indigo, albuminous substances, are common in animals and vegetables; and at length we have arrived at the fact that no distinction can be drawn between the three kingdoms of nature. In the body salt and phosphate of lime and phosphate of soda are animal substances as much as fibrin and albumen. Sugar is as much an animal substance as albumen is a vegetable substance, and no separation can be made by chemical analysis between animal, vegetable, and mineral."

The inquiry leading to the discovery of quinoïdine originated in experiments on the diffusion of quinine through the animal tissues, being a continuation of that relative to chemical circulation by osmotic action which we noticed in our 'Review' for October 1865. From the results of numerous and very carefully conducted experiments, he comes to the conclusion that quinine when used, that is, taken internally, "goes everywhere," even into the crystalline lens, and that wherever it goes it meets with the natural fluorescent substance like quinine, which most probably is constantly forming and undergoing oxidation. And, referring to its medicinal effects, he adds—

"The incoming quinine causes a temporary excess of quinine in the textures. Probably it causes a stoppage of the fresh formation of quinine from albumen, a temporary arrest of the changes going on, a transfer of action probably to the quinine introduced, so that with large doses deafness and great prostration and almost imperceptible pulse are produced in man, whilst in guinea-pigs death even is caused by the extreme prostration. In small doses quinine, probably like alcohol, gives an immediate stimulus when the first chemical action takes place; but soon the quinine retards the chemical changes in the nitrogenous substances, just as alcohol, by its secondary action, retards the chemical changes in the hydrocarbons in the different textures."

From the ascertained facts, viz., the results arrived at, the author looks forward to two hopeful prospects—one the explanation and cure of ague, the other the treatment of diseases in parts of the body external to the blood-vessels, such as the crystalline lens. We quote his words in explanation:

> "1. Assume that a substance like quinine exists in health in the textures, can its rapid destruction and removal through the action of marsh miasm give rise to ague? Does quinine cure ague by furnishing a substance which retards the changes which go on in the textures?—and in the well-known property of arsenic to preserve organic substances have we also the explanation of its power in curing ague?
>
> "2. If chemical circulation can carry alkaloids even into the non-vascular tissues, is it not reasonable to suppose that medicines pass through the blood and act on the textures? And is it not most probable that they take part in every chemical change that occurs outside the blood-vessels, as well as in the blood itself? Still further, may we not expect that among the multitude of new substances which synthetical chemistry is now constantly forming, some medicines may be discovered which may not only have power to control the excessive chemical changes of the textures in fevers and inflammations, but may be able to remove the products of insufficient chemical action, even in those diseases which affect the non-vascular textures, as, for example, in cataract and in gout?"

To those of our readers who wish for the fullest information on the subject, we would refer them to the 'Proceedings of the Royal Society' for April, in which is a paper by Dr. Bence Jones and Dr. A. Dupré, entitled "On a Fluorescent Substance resembling Quinine in Animals; and on the Rate of Passage of Quinine into the Vascular and Non-vascular Textures of the Body."

On the importance of the researches detailed in these remarkable papers we need not dwell. Whether we consider what they have already brought to light or the hopes they encourage of further progress in vital economy, we are tempted to hail them as constituting a new era in medical science.

PART THIRD.

Original Communications.

Art. I.

Observations on (so-called) Nervous Apoplexy, on Congestion of the Brain and Serous Effusion. By W. Boyd Mushet, M.B., M.R.C.P.

(*Concluded from* vol. xxxvii, p. 507.)

Professor Ackermann of Rostock some time since made experiments with the object of ascertaining the condition of the cerebral circulation in asphyxia, which afforded results also opposed to received opinions. He removed a portion of the skull of an animal with a trephine and substituted a piece of glass. After about twenty-four hours, asphyxia was induced by strangulation, submersion, compression of the thorax, &c., and the condition of the brain watched through the glass plate. Asphyxia was found to be invariably accompanied by a condition of anæmia instead of congestion. The congestion of the cerebral vessels, not unfrequently presented after asphyxia, he believes to be attributable to hypostatic hyperæmia of *post-mortem* character ('Archives of Path. Anat.,' quoted in 'Brit. Med. Journ.,' April, 1861).

It would, therefore, appear that in many cases and forms of fatal coma, usually referred to, or said to be attended by, congestion, the latter phenomenon is rather incidental than invariable. If present it must be regarded as a consequence. Opium, alcohol, chloroform, and carbonic acid cause insensibility from toxæmic influence; not from mere induction of congestion. The congestion is secondary from dissemination of poisonous elements in the cerebral vessels, ensuing on diminution and arrest of the normal circulating forces —in like manner as in the lungs, asphyxia is not due to congestion, but congestion to the asphyxia or apnœa.

As I have adverted to the lungs it is not irrelevant to observe that *pulmonary apoplexy* (apoplexie pulmonaire foudroyante) has occasionally proved fatal in a sudden manner and been mistaken for cerebral apoplexy. On subsequent examination the brain and its vessels have appeared normal, but lesion and hæmorrhagic infiltration have been discovered in the pulmonary parenchyma. Such cases are recorded ('Dict. de Méd.,' art. Apoplexie du Poumon) by Corvisart, Bayle, and Andral, where death was almost instantaneous.

In recognition of the above, and that the most eminent of our predecessors often instituted an inspection of the brain only in such and analogous cases, we can repose but little reliance on their statements and researches, although their observations have doubtless exercised important influence on the views and practice of succeeding generations of physicians. I speak not in depreciation of the labours of our illustrious forefathers, as few would accord them merit more liberally, or be more sedulous to vindicate their learning and fame. The advance of scientific medicine has been of necessity gradual, and effected by accumulation of legitimate inference and theory, grafted on the facts of physiology and pathology. "Necessitas medicinam invenit, experientia perfecit. Quæ quidem primâ ætate rudis erat ac stupida; progressu verò temporis accedentibus in dies novis observationibus, sibique mutuò facem quasi præferentibus, cuncta præsertim regente, ac moderante rationis lumine, liberalis facta est et erudita." (Baglivi, 'De Praxi Medicâ,' cap. ii.)

It is, nevertheless, unquestionable that the promulgation and adoption of hasty assumption and unauthorised speculation, even amongst the ablest, have tended enormously to retard its progress. Much incorrect doctrine has sunk to oblivion, much has survived to our day. Owing to the inexactness which more or less enshrouds our knowledge of disease and medical practice generally, divers current dogmata have been taught and circulated, *ex cathedrâ*, from professorial chairs, which are rather the expression of time-honoured error, or the phantoms of preconception, than the offspring of direct investigation, though most obsequiously assented to by particular sects of past and present practitioners. Even the distinguished Laennec believed that *congestions* arose, under certain circumstances, in consequence of the blood experiencing a rapid dilatation. On no other explanation could he account for the immense losses of blood sometimes occurring in hæmoptysis or menorrhagia, or the sudden hyperæmiæ of internal and external organs witnessed in epilepsy and hysteria.

I have admitted that under certain conditions, especially traumatic, apoplectiform symptoms are undoubtedly presented as the consequence of pressure, but that such pressure is exercised through the medium of congestion is void of proof or probability. Pressure evokes disorder of nutrition, either from direct effects on the nervous centres, or from alteration in quality of the blood, from stagnation and arrest of the physical changes—a state more allied to anæmia than congestion. The physiology of sleep, the effects of profuse hæmorrhage, observations on asphyxia and the action of anæsthetics, with many other trivial facts warrant the conclusion that unconsciousness is more frequently attended by a bloodless than a congested condition of the brain.

Pressure induced by external lesion is almost invariably complicated by concussion, which alone, in absence of compression, is able to exert most of the phenomena of pressure, demonstrating that changes apparently inappreciable, in the (polarised) constitution of the brain are equally represented by symptoms commonly attributed to mechanical causes.

Mr. Abernethy, in 1797, in his surgical and physiological essays (Part III, on 'Injuries of the Head'), published a series of cases in proof that extensive depression of the cranium—of the parietal, frontal, or temporal bone—might exist unattended with coma, and that recovery might take place without trephining, the skull remaining depressed. In *one* or *two* instances only there was transient stupefaction from the injury, which may, with fairness, be referred to shock or concussion rather than compression. The inevitable inference Mr. Abernethy drew from these cases was that pressure does not always derange the functions of the brain, either at the time or at any remote period.' On the other hand, Mr. Abernethy points out that severe symptoms sometimes ensue when the bone is elevated and pressure absent. He remarks that a surgeon might be led to ascribe the mischief in these cases to the depressed bone, if the trephine were not employed; and the success in those recovering without operation to the same instrument, if elevation had been practised. In conclusion, I think it impossible to maintain that vascular congestion or pressure is the *ordinary* origin of apoplexy or comatose symptoms, to the exclusion of nutritive alterations or perversions. Coma is frequently developed when states opposite to pressure or congestion are established, as in syncope, concussion, hydrocephaloïd, or pseudo-meningitis, sudden hæmorrhage and ligature of the carotids; and also when there is no evidence of congestion or abnormal pressure, as in ramollissement, uræmia, cerebral embolism, delirium tremens, fever, and during the incubation and progress of certain acute and specific diseases. On the contrary, in cases where pressure must exist there may be entire negation of comatose symptoms; and, in others, the post-mortem discovery of cerebral congestion may have been unpreceded by coma or any indication connected with the nervous centres. Excessive action of the left heart—if injurious where the minute vessels of the brain are sound—syncope or arrest of blood to the head produce effects, not from variation of pressure, but from modification of the process of nutrition. In such circumstances, by the upholders of the *pressure* theory, we are not enlightened why the cerebro-spinal fluid fails to afford the succedaneous efficacy with which it is, on all other occasions, physiologically invested. After I had ventured to commit myself to the above novel and heterodox views, I derived some complacency from ascertaining that Dr. Wood of Philadelphia has in some measure similarly expressed himself.—"Some appear to consider coma as dependent upon pressure alone. But as this cause operates merely by suspending certain functions, there is no reason why stupor, in its different grades, should not arise from other causes capable of interrupting the same functions, whether by an excess of excitement or a direct sedative impression. I have no doubt whatever that it often results from both these causes, altogether independently of pressure." (4th edition, vol. ii, p. 683.) Yet the most recent authority, Dr. Maclachlan, follows the routine opinion. Thus—"Apoplexy appears in very opposite states of the system, or condition of the brain itself in regard to plethora or anæmia, but the *tangible* cause of the disease is pressure" (p. 133). I would rather say, *intangible*, and Dr. Maclachlan, in a previous sentence, admits that "the proofs of the existence of pressure, in every case, are nevertheless very defective."

Reference to the *quality* of the vascular supply, toxic or spanæmic, or nervous disturbance will, in all cases, satisfactorily elucidate the seeming contradictions and be equally explanatory of vertigo, delirium, epilepsy, puerperal eclampsia and insanity—not to mention emotional states—which are incapable of solution on mechanical considerations or supposition of congestion.

Serous or pituitous apoplexy is a variety of the disease which has been for ages, and is even at the present day, recognised as a substantive cause of death amongst infants and adults by a large number of practitioners. It is a common verdict of coroners' juries, whose finding must depend on medical evidence, which for the most part merely testifies to the presence of serum in the cerebral ventricles, arachnoid cavity or sub-arachnoid space of the deceased.

I have been long led to conclude that, independent of the cause of death, it is more usual than otherwise to meet, in the adult, with some amount of fluid, increasing with age, in the lateral ventricles or at the base of the brain, and that its presence has no fixed relation to the degree of vascularity, or engorgement of the brain or its vessels. In young children, however, dying from diseases not of the comatose class, I have more frequently than in those of later years, remarked the absence of fluid from the ventricles and the base of the brain. On reference to my register of post-mortems in persons over fifty years, not dying of apoplectic or uræmic disease, I find the following account of the fluids within the skull, when they have been noted.

T. M., 65. Brain healthy, serum in ventricles.

J. M., 60. Brain not congested. About ʒij of serum in each ventricle.

J. R., 73. Membranes healthy, about ℥j of serum in each ventricle and fluid beneath arachnoid.

E. H., 78. Vessels very turgid, and substance of brain vascular. Small amount of fluid in ventricles, and about ℥ij at base of skull.

J. R., 54. Brain slightly extra-vascular. Sub-arachnoid effusion and a little fluid in ventricles and at base of brain.

W. J., 52. Ventricles contain moderate quantity of serum.

—, 55. No fluid in ventricles. This was a case of drowning, and in another female, aged 30, also drowned, there was no fluid in ventricles, and in both the brain-substance was natural, with fluid blood in sinuses.

—, 60. Brain-substance natural. Ventricles distended with serum, and about ℥ij at base of skull.

It has been said that accumulation of fluid in the ventricles causes coma, whilst sub-arachnoïd effusion has no such tendency, but in chronic hydrocephalus there may be serum in the ventricles from the commencement, with persistent absence of comatose symptoms. I have, moreover, met with cases of coma, not dependent on cerebral hæmorrhage or uræmia, in which there was no fluid in the ventricles, and in the cases of drowning above cited it was absent. In cases of sudden death it may be or not present; and in many cases, even of uræmia (or chronic Bright's disease) there may be neither serum in the skull nor any trace of anasarca. Whether death be due in these to urea or spanæmia (whether the blood be toxic or defective, or suffer from a combination of evils), it is at any rate from alteration of the *quality* of the blood, not from serous effusion or pressure.

Nevertheless, on consulting many eminent authorities on the subject of serous effusion, I was surprised to discover that they greatly accorded, and confirmed my own previously-espoused views, which, I imagine, notwithstanding, to be at variance with widely-accredited doctrines. Abercrombie thought it was in the highest degree improbable that serous effusion should occur in the brain as a primary disease and accumulate with such rapidity as to produce the symptoms of an apoplectic attack. He observed that the quantity of fluid effused bears no proportion to the degree of the apoplectic symptoms. It is found in small quantity, though the apoplectic symptoms have been very strongly marked and long continued; it is found in large quantity where the symptoms have been slight; and finally there may be most extensive effusion in the brain, when there have been no apoplectic symptoms at all. The direct inference he drew from these facts was that in the cases of apoplexy with effusion, the presence of the fluid cannot be considered as the cause of the apoplectic symptoms (p. 216).

Dr. Clutterbuck likewise considered that serous effusion was not the cause of the apoplectic symptoms, but that these were produced by the morbid condition of the brain, on which the effusion depended.

Dr. Cooke ('Nervous Diseases,' vol. i, p. 268) thought we must admit that the serous apoplexy very seldom occurs, and adds (p. 341)—"I have never seen a case exactly answering to the description of this form of the disease." Dr. Sims ('On Serous Effusion from the Membranes and into the Ventricles of the Brain,' Med.-Chir. Trans., vol. xix.) pointed out the very great frequency of collections of serous fluid in the ventricles or membranes of the brain, in cases, at all ages, where death occurs from diseases not cerebral, and where no cerebral symptoms are known to have existed. Without denying that such a disease as serous apoplexy may occur, he thought, at least, that it was extremely rare.

Andral believed it by no means well-determined how far the effusion is concerned in the production of symptoms, as it often appears unattended by them, whilst, on the other hand, we frequently observe these symptoms in cases where, after death, there is not any effusion, worth mentioning, to be found either in the ventricles or elsewhere.

Dr. Todd ('Cyclo. of Anat. and Phys.,' art. Nervous Centres) says it is in vain for the pathologist to attempt to form an opinion respecting the quantity of the fluid found in the cranio-spinal cavity, unless the inspection has been made at an early period after death. Practical men are too much in the habit of attributing morbid phenomena of the nervous system to the influence of the pressure of a liquid effusion upon the brain or spinal cord. Many facts tend to show that in a large proportion of cases, especially in the adult, the occurrence of an increased quantity of fluid, either around those centres or within the ventricles, is a *result*, and that probably it is a result of a *conservative fluid*, consequent upon a morbid change which depresses the general nutrition of these organs themselves. Dr. Hughes Bennett again notices that effusion of serum is not unfrequently found after death from apoplexy. He ascribes it to obstruction of the blood and transudation of the more fluid parts through the coats of the vessels, the same as in other dropsies. He states that it is seldom found without engorgement of the veins and sinuses, and that it may take place immediately before or even after death.

Dr. Watson imagines that a moderate quantity of serous fluid poured out rapidly during life would certainly exert a degree of pressure adequate to the production of fatal coma, though how the serum comes to be so effused, he admits, it is not easy always to say.

Dr. Burrows does not think serous effusion to be a cause of coma, but vascular congestion. Dr. Copland does not consider the serous effusion to be the cause of the apoplectic seizure, but the consequence of that state of the circulation on which the disease more immediately depends. Indeed, he is of opinion that a considerable portion of the effusion takes place just before death, or soon after life is extinct; and he remarks that in apoplexy presenting on dissection *congestion* and serous effusion, these states may be often considered rather in the light of *post-mortem* changes than the pathological states, which had existed previous to death. According to Dr. Sieveking ('Path. Anat.,' p. 222) there is no doubt that occasionally the sub-arachnoid fluid is attributable to cadaveric changes; it is, therefore, necessary to be circumspect in at once attributing its presence to antecedent morbid action. The amount and position, and more particularly the concomitant appearances of the pia mater and arachnoid must assist us in determining the question in the individual case.

Dr. John Reid (loc. cit.) who offers no speculations, observes that the condition of the brain which causes the symptoms in (so-called) serous apoplexy, has not yet been fairly elucidated.

Sauvages, exactly a century since, in his 'Nosologia Methodica,' although his views were, of course, not untrammelled by the speculative and defective pathology of the period, clearly expresses his belief that the presence of serum within the skull afforded no precise indication of the cause of the apoplectiform symptoms. "Quòd in cadavere sinus cerebri aquâ turgidi reperiantur, non sequitur apoplexiam ab illo sero fuisse inductam; cum ubi infarctus est vasorum sanguiferorum, ibi lympha facile e vasis suis elabatur, et quo longiori post obitum tempore sit extispicium cadaveris, eo uberior est ut plurimum seri effusi copia; vidi hydrocephalos ingentes sine apoplexiâ, viderunt et alii: veruntamen hæc species est omnium funestissima, quia labem inveteratam in cerebro, ac sæpius ætatem senilem supponit, in quibus casibus facultas motrix quæ sola morbis medetur, efficaciter est effæta et languida" (p. 498).

Henceforth, I trust that not any medical practitioner, if he meet with even copious accumulation of serum in the ventricles of the brain, or at the base of the cranium, will have the temerity, in default of knowledge or history of the case, to assign death to serous effusion. Nay, *if he be* aware that comatose symptoms have pre-existed, that he will yet refrain from estimating the presence of serum—as a *primary* morbid act or sequel of cerebral congestion—to be adequate explanation of the fatal event, without inquiry or reflection, as to the cause of the effusion, and vigilant search for morbid alterations in the brain and other organs. By strict observance of these cautions, I am persuaded that "serous apoplexy" as a primary disease would almost disappear, if it were not actually expunged, from our nosological systems.

In further proof that I stand not isolated in these views, which will, I am assured, become general with increasing knowledge, I may state that Dr. Abercrombie (p. 145) maintains that we have no certain mark upon which we can rely as indicating the presence of effusion in the brain, and I beg to requote Dr. Maclachlan, a physician, be it remembered, ripe with over twenty years' experience amongst the Chelsea pensioners, who doubtingly asks (p. 146), Is there such a disease as serous apoplexy? This inquiry is, to my mind, an emphatic declaration that Dr. Maclachlan has never been able satisfactorily to recognise intracranial effusion as a primary and efficient source of coma. Mons. Littré (loc. cit. prop. xvi), in 1834, also dubiously inquired—Existe-t-il des apoplexies séreuses? a question which Cruveilhier ('Dict. de Méd. et Chir. pratiques,' art. Apoplexie, prop. xvi, 1829) had uttered some years before, and was inclined to answer non-affirmatively.

Dr. Copland, in his Dictionary, does not treat of serous apoplexy under a distinct head, nor does Dr. Aitken, more recently, in his 'Practice of Medicine.'

Rokitanski ('Trans.' vol. iii, page 404) hesitates whether such a disease as serous apoplexy really exists, and whether it can be recognised in the body by the *post-mortem* appearances without reference to the symptoms attending the death of the individual.

To turn to recorded cases. Those of serous apoplexy described by Morgagni ('De Apoplexiâ serosâ,' lib. i, epist. iv) are unable to support the title, and at the commencement of his epistle he observes—"Nec vero nos ii sumus qui quotiescunque intra apoplectici calvariam aqua invenitur, continuo ab hâc ejus morbum repetendum esse, existemus." To select a few of the most prominent. A case of acute fever (febris acuta), Art. VI; sudden death during illness, Art. IX; man of seventy found dead, Art. XI; fatal drunkenness (?) in a young man, Art. XVI. Not to be prolix, many of the accounts are most meager and the inspection most unsatisfactory, being confined to notice of the serum. All the cases, I think, are referable to causes other than mere serous effusion as the proximate morbid action, and Dr. Cheyne thought ('On Apoplexy and Lethargy,' p. 48) that much objection lay against the cases of Morgagni as specimens of serous apoplexy. Dr. Cheyne considered that the causes of serous apoplexy are involved in great obscurity, and of its symptoms and anatomy he knew little more than the one case afforded (XVI) which is related in the 4th section of his work. This case may be found in Copeman's 'Collection of Cases of Apoplexy" (the 97th), and appears to have been one of complicated meningitis.

Dr. Bright (vol. ii) gives a number of cases (CIII to CXXIV) to illustrate the pressure arising from serous effusion independent of inflammation:—

CIII. Effusion under arachnoid and into ventricles after hanging.
CIV. Cerebral congestion from suffocation.
CV. Do. do.

CVI. Serous effusion from suffocation.
CVII. Do. do.
CVIII. Cerebral congestion from suffocation.
CIX. Effusion under arachnoid and into ventricles in disease of heart.
CX. Congestion and effusion in phthisis.
CXI. Effusion under arachnoid; renal disease.
CXII. Congestion and effusion; renal disease.
CXIII. Effusion, &c. do.
CXIV. Effusion, &c. do.
CXV. Effusion, &c. do.
CXVI. Effusion, &c. do.
CXVII. Effusion, &c. do.
CXVIII. Effusion in a man with tubercular lungs.
CXIX. Serous effusion under arachnoid in a case of emaciation, with vomiting and disease of supra-renal capsules.
CXX. Effusion in a hard drinker with tubercular lungs.
CXXI. Hemiplegia; old disease of brain and effusion into ventricles.
CXXII. Serous effusion in diabetes; death from gangrene of the lungs.
CXXIII. Effusion in diabetes.
CXXIV. Do.

Now, these cases, which are the whole under the head noticed in Dr. Bright's work, give no support to the view of serous effusion being a primary and substantial disease. The effusion, *in all cases*, was associated with well-marked morbid processes, as with hanging, suffocation, cardiac incompetency, phthisis, *uræmia*, exhaustion, disease of the brain, and diabetes. In many of the subjects the effusion was evidently *apud*, or *post-mortem*, and from the symptoms it is clear that it contributed no share to the production of the fatal issue. To verify with exactness in all cases, the conditions under which collections of serum within the cranium are accumulated is, however, more difficult than to elucidate that they are insufficient to produce comatose symptoms.

I believe that the amount of serum within the skull is usually found to be more limited, relatively as well as absolutely, in young subjects, and that the quantity seems to be almost irrespective of the direct cause of death, though perhaps influenced by mode and rapidity of dying and fulness of the vascular system. In aged persons, in whom apoplectiform attacks are most common, the presence of serum even in large (but varying) amount is the normal consequence of shrinking or condensation of the cerebral substance, or atrophy. Even in uræmia the attendance of serous effusion is not invariable, and if pre-existent it is unconnected with the induction of unconsciousness.

It is impossible accurately to estimate, says Dr. Maclachlan, the normal amount of fluid in the ventricles and beneath the arachnoid in any one instance; it is often difficult to say that it is increased in quantity. This is more particularly the case with that contained in the sub-arachnoid space. When it exceeds three drachms in either lateral ventricle it may be regarded as abnormally augmented. It is generally in both situations greatly increased in old age, and there can be little question that its influence in the production of apoplectic affections has frequently been over-rated (p. 96).

The senile changes in the brain, which consist in wasting or diminution in bulk of the encephalic structures, necessarily require increase of the cephalo-rachidian fluid to maintain a *plenum;* and pathologists familiar with autopsies in the old, amongst whom I may mention Rokitanski, Drs. Sieveking and Maclachlan, allude to the augmentation of the cerebral serum in advanced life. This is often denominated "senile hydrocephalus," an accumulation which has long existed, and to which death can in no way be ascribed (Maclachlan). In such cases the arachnoid, subarachnoid space and ventricles of the brain may be distended with fluid, so as in extreme instances to attain to as much as twelve ounces in quantity. In hydrocephalic cases, in young adults, even a gallon of fluid has been collected within the cranium without supervention of coma.

Now these accumulations, as in infancy, are undoubtedly gradual, are conservative, and in nowise indispensably connected with altered pressure or apoplectiform symptoms. If coma supervene it is frequently to be traced to uræmic intoxication; and albumen may be detected on catheterism, which should always be performed, and urea discovered in the serum from the nucha after vesication, and post-mortem in the ventricular effusion. After death, as Dr. Basham cautions us, the contents of the bladder are universally albuminous.

Should the kidneys be sound, the symptoms are not to be referred to the effused serum, or pressure, but to arrest of nutrition and of function from softening or atrophy of the brain; or obvious explanations of death may be presented in the other cavities, *i. e.*, dependent on causes enumerated under "nervous apoplexy" and "congestion of the brain," as cardiac or other disorder.

In testimony, even in our day, that a renal causation may be readily overlooked, I may mention that I have seen a physician of long hospital experience confound a case of tolerably typical uræmia with local cerebral disease. It is certain, also, that some inflammatory cerebral complaints, as meningitis, simple and tubercular, have been set down, when proving fatal, to serous effusion. Thus, until within the last thirty years, tubercular meningitis in children was generally regarded as essentially a dropsical effusion within the brain, and was in consequence designated "hydrocephalic apoplexy," "water stroke," or "acute hydrocephalus." These erroneous views are not yet universally eradicated, although, like uræmia, the disease may terminate in death without the occurrence of effusion. Many of the cases of Dr. Abercrombie, under the title of acute hydrocephalus, are evidently tubercular, as there is history of phthisis; and most are in children. This physician upheld that it was not the mere presence of a certain quantity of fluid in the brain which gives rise to the symptoms of hydrocephalus, as he had seen the disease pass through all its stages, and end fatally without any effusion (p. 144). Under the head of apoplexy Dr. Abercrombie relates only three cases of serous effusion. In case cii a man of eighty, the symptoms appear to have arisen from uræmia, or senile softening. In case ciii, also uræmic or cardiac (?), it was a man of seventy; and in case civ, a dropsical patient aged forty-one, the attack beyond question is ascribable to renal disease. It may be noted in corroboration of these conclusions, that in each instance the coma was not profound. It were unnecessary to recount the many forms of coma in which, after death, serum may be discovered on examination of the cranium. I may, nevertheless, repeat that it is very often met with in the diseases alluded to under nervous apoplexy, and congestion of the brain, especially in uræmia, inflammatory and chronic cerebral disease, delirium tremens, and, more or less, in most elderly subjects, and after exhausting diseases. It may be found effused at any age and under the most varied circumstances, which, I imagine, it is not requisite to consider further, as I am most concerned with its presence in the cerebral cavities of those cut off by that species of coma commonly known as 'serous apoplexy.' I will, therefore, relate a few histories in persons past the prime of life, which tend to prove how frequently an apoplectiform attack (in which no morbid appearance in the brain but intracranial serum may be revealed on inspection) depends on extensive structural changes in the kidneys. In such cases, the invasion of uræmic intoxication may be very sudden, the patient having never complained of symptoms, or at least having never brought them under medical observation.

Case 1.—An old man, with prostatic enlargement, who had been accustomed to draw off his urine once or twice daily, with an elastic catheter, was attacked by erysipelas of the leg, which confined him to bed. A considerable amount of urine dribbled away, and as attention was not drawn to the condition of the bladder, and he had in the mean time ceased to introduce the instrument, he became by degrees unconscious, which was at first referred to his recent malady. Such a case, which might lead to coma or urinary mischief, is more likely to happen in a public institution than in private practice, as amongst the better class of patients there is greater eagerness to live, and the condition of the urinary organs, if impaired, is certain to be brought prominently under notice.

The case is alluded to, as, if proving fatal and serum were subsequently discovered in the brain, death might be directly assigned to the effusion.

Case 2.—A woman, æt. 58, who had been two or three times under treatment for sub-acute rheumatism, was brought to the infirmary in an insensible state. The pupils were dilated, insensible to light; the limbs rigid without paralysis; mouth drawn to right side. The complexion was pasty, pale; there were twitchings of the facial muscles and grinding of the teeth; slight stertor, and the lower extremities moved on pinching the skin of the leg, but tickling the soles did not produce reflex movement. There were appearances and expression about the patient as of feigning insensibility. Pulse was feeble. According to report she had had epileptiform attacks during the day. There was no further history. There was also very slight œdema of the ankles, and the urine was found to be highly albuminous, and contained casts.

She was cupped over the loins, a blister was applied to the nucha, sinapisms to the calves, and croton oil was administered, but she died ten hours after admission.

Post-mortem thirty-three hours after death. Rigor mortis well marked. No arcus. Brain nothing marked, but substance very firm. About an ounce of serum in sub-arachnoid space. Soft commissure very noticeable. Slight opalescence of arachnoid; vessels of brain apparently not diseased. Lungs congested and emphysematous. Bronchial tubes healthy. Heart, ten ounces; cavities contain dark and decolorised coagula in all chambers, most right. Surface encroached upon by fatty substance. No valvular disease. No fluid in pericardium. Liver large, yellowish-brown; centre of lobules pale; periphery injected. Kidneys weigh three and a half ounces each. They are lobulated and granular on section. The capsule, on removal, brings away renal substance. Cortical substance increased; medullary, at parts, almost obliterated. They are highly granular and contracted in appearance, after the removal of the capsule. Stomach contracted. Spleen small and firm. Other viscera normal.

Case 3.—A washerwoman, æt. 64, was admitted into St. Marylebone Infirmary. She was pale, cold, haggard, and I was informed that she had had a fit a short time previously. There was sopor, but not actual coma. The pulse was 100, full, weak, compressible. Respirations 11 to 12; noisy, gasping, with loud râles. Pupils were contracted and insensible to light. Arcus was very strongly marked. Considerable œdema of feet and legs. Mouth deviated, but there was neither paralysis nor convulsion.

She was wrapped in blankets, hot bottles were applied to the feet, a blister to the nape, croton oil was given, and she was ordered beef tea at intervals.

Next morning she was completely comatose. Pupils still contracted; surface warm; mouth drawn to left side. The face was somewhat livid. The action of the heart was weak and rapid, the pulse almost imperceptible. Respirations 18. Bowels had not acted. Two pints of urine were withdrawn, which, contrary to injunction, were thrown away. She could not swallow, and was evidently sinking. She died in the evening, twenty-two hours after admission. Autopsy thirty-six hours afterwards. No external marks. Œdema of legs and feet. Pupils moderately dilated. Rigor mortis tolerably well marked.

Head.—Scalp and calvaria natural. Dura mater injected. Some sub-arachnoid effusion, and two to three ounces of fluid at base of skull. Arachnoid partially opaque. Prominence of large veins of pia mater. Brain-substance healthy, but puncta vasculosa unusually evident and numerous. About the centre of the brain, the substance a little softer than natural. No clot, no injury to skull. Some serum in the lateral ventricles. Vessels at base extensively atheromatous.

Heart.—Sixteen ounces, surrounded by fat. Hypertrophy of left ventricle, and aortic orifice narrowed from binding down of one of the semilunar valves. Mitral valve appears healthy; but the aperture is constricted, admitting only one finger. The aorta is dilated above its valves. Valves on right side normal. A partially decolorised clot in right auricle, extending into ventricle and interlacing with its fleshy projections. Very little blood on left side of heart. Coronary arteries atheromatous. No fluid in pericardium. Lungs healthy, with tenacious yellow mucus in trachea, bronchi, and divisions. Some serous effusion into right pleura, no adhesions. Left lung slightly adherent to costal pleura by recent exudation; and effusion into chest, on left side, of some ounces of turbid serum, holding in suspension flocculi of lymph. Liver healthy but pale. Spleen small, firm. Stomach contains some ounces of greenish fluid and much mucus, without peculiar smell. Internal surface healthy. Gall-bladder distended with dark, oily bile. Right kidney three ounces; small, hard, shrunken, granular, lobulated, and pale. The capsule separates with difficulty. Left kidney a little over two ounces, with the same characters to greater extent. Much fat around viscera. No fluid in peritoneum.

CASE 4.—J. C—, æt. 79, died on February 15th. She was taken with a fit about 1 a.m.; apparently uræmic, and died in about four hours.

Post-mortem three days afterwards. Weather intensely cold. Face placid, pale. No external marks. Body tolerably stout. Pupils moderately dilated.

Head.—Dura mater adherent to calvaria. Some congestion of surface of brain, and some fluid between arachnoid and pia mater; the former opalescent at spots. Brain-substance very firm. Ice (frozen serum) in ventricles slightly sanguinolent. Much fluid at base of brain. Extensive atheroma of cerebral arteries, but no calcification detected. Dark clots in lateral sinuses.

Heart.—No valvular disease, and it appears to be perfectly healthy in size, neither hypertrophous nor dilated, although the lungs are extensively emphysematous, especially along the borders, and at bases in contact with the diaphragm, some of the sacculi being as large as a damson. Coronary arteries of heart atheromatous. Left cavities filled with dark coagulated blood. Slight amount on right side. A little fluid in pericardium.

Bronchi and divisions give evidence of chronic bronchitis, containing muco-purulent secretion. Stomach empty, contracted, pale. Kidneys large, pale, granular, and adherent to capsule. Uterus very hard. Other viscera normal.

Case 5.—S. K—, æt. 78, died on May 29th, with marked symptoms of uræmia and highly albuminous urine.

Post-mortem two days subsequently. Weather fine. Look placid, lividity of nails, much sub-cutaneous fat. Pupils moderate. Arcus well marked. Slight œdema of feet and legs. Rigor mortis well marked. Calvaria tolerably thick. Dura mater rather injected. Arachnoid opaque, and much serum between it and pia mater; the latter rather congested posteriorly. Brain thirty-nine ounces, rather less firm than natural. Puncta vasculosa, numerous. Effusion of serum into ventricles and at base of brain. Dark semi-fluid blood in sinuses. Vessels at base atheromatous.

Amount of serum in cranium from three to four ounces.

Chest.—Lungs mottled-greyish, emphysematous, imperfectly crepitant anteriorly, and of coriaceous firmness. No fluid in pleuræ. Slight adhesions on right side. No tubercle. Bronchi dusky red, and contain some frothy mucus. Pericardium not adherent, contains an ounce of serum. Heart ten and a half ounces, large and very flaccid, containing much dark clot in right auricle, a little dark blood in other cavities. Orifices appear natural. There is a valvular opening in the foramen ovale, sufficient to admit the tip of the little finger. All the valves healthy. Muscular walls of ventricle are much encroached upon by fat, and the colour is pale, almost straw coloured. Substance is very friable, and exhibits fatty degeneration under microscope. Coronary arteries are cretified and atheromatous, as is also the aorta. Stomach dilated, contains much mucus, its lining pale. Pancreas congested. Spleen three ounces, very diffluent and pale.

Right kidney, weight three and a half ounces; left, three ounces three drachms. They are rather small, pale in colour, firm and markedly granular on separation of the capsule, removal of which detaches the subjacent tissue. The cortical portion in some parts is not easily distinguishable from the medullary; the latter better marked and more natural in certain places. Microscope reveals fibro-plastic granular and fatty matter, but no casts.

Liver thirty-five ounces, friable, rather small; the centre of the lobules dark, the margin pale. Bile light coloured and small in quantity. Intestines appear healthy. Uterus one ounce. Fallopian tubes seem impervious. Other viscera present nothing marked.

Case 6.—I relate the following case in this place as it is one of much interest, and Dr. Richardson records one, not unlike it, under "uræmic coma," which appears to have been benefited by bloodletting. F. S—, æt. 58, pale and sallow. Has been a painter, but for the last three years in declining health, and very subject to gout and asthma, the latter coming on occasionally in most distressing and threatening paroxysms. States that he has never had colic or wrist-drop or other effects of lead-poisoning. Has slight arcus on upper half of cornea. Was intemperate in early life.

At the commencement of January, 1856, he was attacked with vomiting, without obvious cause, which was not much influenced by remedies. He had no head-symptoms at this time. On January 12th he was suddenly seized with a fit which, from the description, was of epileptiform character, being attended with complete unconsciousness, eyes turned upwards, general rigidity of the muscles, and foam at the mouth. Three or four days before this fit, convulsive twitches of the face and arms were noticed by the nurse to come on occasionally (three or four times in an hour). After the fit he was in a lethargic state; his speech was less distinct, and there was considerable mental hebetude. The pupils acted tolerably well. His urine was of low specific gravity, clear, straw-coloured, and albuminous. He was ordered compound jalap powder and acetate of ammonia mixture.

Up to January 17th he remained much in the same state, but had one or two fits daily of a similar kind. On the 17th, about six a.m., he was seized with violent convulsive or choreal twitchings of the muscles of the body generally, especially of the muscles of the face and of the upper extremities. The facial muscles were in constant movement, so as to produce a painfully ludicrous effect. He did not appear much distressed by these twitchings, and stated that he felt no pain. They did not succeed a fit, but came on suddenly without loss of consciousness. The tongue participated in the movement. He could protrude it when desired; it was brownish, furred, dry, similarly agitated to the muscles of the body, and did not deviate. Respirations 32, shallow, gasping, chiefly abdominal. The expression was somewhat anxious, but not so greatly as might be inferred. Consciousness was much impaired, but some replies were more rational than others. Common sensibility was also impaired, he did not or did not appear to feel smart pinching. Tickling the soles excited but little movement. He could rise in the bed when his legs were examined, which were not œdematous. There was no paralysis or rigidity. Pulse 96, feeble. He had not, and had never complained of, pain in the head. When asked, he stated indistinctly that the twitchings did not hurt him, in fact, that he did not feel them.

On the 18th he was seized with a fit during my visit. His head was suddenly drawn to the left, the movements increased in severity and rapidity, and general contraction of the face and trunk ensued. The muscles of the body became tonically contracted, so that he did not move in the bed, but was tetanically extended, the legs and arms likewise becoming rigid. The eyes were turned upwards; the face became of a dusky, pallid, death-like hue, not flushed. The tongue was not protruded nor the jaws ground together, but some foam escaped from the mouth. There was no stertor. The attack lasted about five minutes, the muscles relaxed, and coma and stertor remained, with return of the twitchings and general flaccidity of the limbs.

A few hours after I ordered him croton oil and a blister to the nape.

January 19th.—No more fits. The medicine has acted thrice. Has had no sleep and the twitchings have continued, being at the present time stronger than yesterday, but confined to the face and upper extremities. His appearance much resembles that of a person recently dead subjected to galvanism. An attack of asthma has come on, and without aid he had got up and sat on the side of the bed in his shirt, breathing 62 per minute. The surface was covered with perspiration. He answered "no" to all questions, and consciousness was considerably impaired.

He had no dysphagia, and had taken bread and milk, beef tea, eggs, &c.

He swallowed an ether draught, and soon afterwards moved voluntarily into bed. At 12½ p.m. I was summoned, and found him just dead. The twitchings ceased about a quarter of an hour before death, and he sank gradually, withont convulsion.

Post-mortem, twenty-one hours after death. Weather warm. Face pale, placid; pupils not so dilated as immediately after death. Rigor mortis well marked. No œdema. Muscles of good colour.

Head.—Calvaria moderately thick. Dura mater very adherent to the skull and removed with it, but detachable by force. Lateral sinuses contained partly decolorised firm clots. About an ounce of reddish serum at base of skull. Vessels extensively atheromatous. Considerable sub-arachnoid effusion, so as to lift the membrane from the surface of the hemispheres. Arachnoid greatly opalescent, especially over the anterior lobes of brain. Vessels of pia mater not much congested. Brain-substance very firm, and very vascular on section, showing numerous bloody points, and the whole of the white substance had a slight but distinct pinkiness of hue. Ventricles contained but slight quantity of fluid, about a drachm in each. No abnormal appearance about thalamus, corpora striata, or other parts of brain. Brain weighed, after having been drained of serum, fifty-two ounces.

Chest.—Heart fourteen and a half ounces. Pericardium contained about an ounce of serum. A large, partly decolorised clot in right auricle; small fibrinous clot in right ventricle, extending up the pulmonary artery. Left auricle contained black clots extending into pulmonary veins, and the left ventricle some dark grumous blood. No disease of valves or orifices, but left ventricle hypertrophied. Right ventricle also thicker than natural. The cavities do not appear much dilated. Muscular substance appears good to the eye. Walls firm. Extensive atheromatous deposit on inner aspect of aorta. Lungs extensively emphysematous and much congested posteriorly. No adhesions, no fluid in pleuræ, no tubercle. Bronchial tubes intensely injected, of a dusky hue, the duskiness becoming gradually paler towards the upper part of the trachea, where the mucous surface is quite pale. Stomach contained bilious-looking mucous secretion. Spleen firm.

Kidneys small and shrivelled, each weighed two and three quarter ounces; they were very similar in appearance as well as in weight. The capsules did not separate easily, but brought off the renal substance in spots. The exposed surface of the kidney was largely granular, whitish-yellow in colour, and exhibiting very distinct mottling of the organ. The feel was very firm, and section gave a very distinct sense of induration of the tissue. The cortical substance had almost disappeared, and the medullary was much paler than natural. The liver pale, with fawn-coloured patches on its surface, extending for about half an inch into its substance.

The entire colon was contracted, being hardly of the calibre of the small intestine. It presented no other unusual appearance, and the remaining viscera seemed healthy.

In recapitulation. It is admitted by every writer who acknowledges a plurality of apoplexies, and refers to frequency, that hæmorrhage into the brain is by far the most common cause of apoplectiform attacks. If to this cause be added uræmia, and those cases in which, on strict inquiry and examination, obvious explanation of death is afforded—as ramollissement, narcotism and other toxæmiæ, tumours, embolism, aneurism, &c.; the remainder will be very limited, in which we have to interpret the fatal event by reference to "nervous apoplexy," "vascular congestion," or "serous effusion," and accompanying (hypothetical) pressure. These are the refuge of ignorance, and made available when the cause of death (in the brain) appears otherwise inexplicable. I ask, is the autopsy always completely performed, and are all the viscera and the vascular system thoroughly examined in these cases? Have they not become much more rarely recorded, with advancing knowledge of renal diseases, more accurate pathology and acquaintance with the phenomena of embolism, cardiac affections, and various blood disorders?

It is indisputable that (so-called) nervous apoplexy *may* depend on uræmia without effusion, on fibrinous occlusion of the cerebral arteries, and on undetected narcotism; not to enumerate cardiac and other diseases.

To accept the coma as symptomatic, is to recognise undeniable facts; to regard it as a primary idiopathic affection leaving no trace of its action, is a baseless conjecture. In these *nervous* cases, we can only allow that there is no anatomical evidence that the brain is the *origin* of the coma. The attack is not to be imputed to an intangible, inconceivable neurosis, but rather to disease of other organs, which should be carefully investigated, and, if necessary, analysed.

As to congestion and effusion, they are often present without antecedent symptoms of brain affection, and often absent after profound and protracted coma. Many hold that congestion tends to disappear after death, as Hope, Bennett, and Burrows, whilst Dr. Copland, on the contrary, thinks congestion partakes much of the nature of post-mortem changes. The appearance of congestion seems to be much regulated by the mode of death, properties, and stasis of the blood, and many causes not satisfactorily settled. It is certain that symptoms almost identical may, in one instance, be observed with congestion; in another, with effusion; in another, with normal appearance of the brain—a weighty token that these appearances are accidental and extraneous.

The vascular condition of the brain varies much during sleep and wakefulness, during rest and exertion, and during intellectual repose and labour. Mr. Durham ('Guy's Hospital Reports,' 1860) has demonstrated, in opposition to Dr. Cappie ('Essays on Medical Science,' 1859), that venous pressure is not the cause of sleep. On the contrary, there is diminished quantity and velocity of blood in the brain.

Serous effusion, in senile and infantile hydrocephalus, may persist without interfering with consciousness. By most reflective men it is regarded as the *result,* frequently post-mortem, of other operations and not directly connected or associated with coma, as it may be absent after death from coma, even in that class of cases wherein effusion is more commonly detected; and it may be presented in large quantity where consciousness remains until the last moment of life. On the whole, effusion appears to be more indicative of secondary transudation than of primary cerebral dropsy causing coma.

The pathological facts divulged by cerebral diseases on post-mortem examination, conjoined with their symptoms and phenomena during life, afford an irresistible demonstration that pressure is absent in a vast number of comatose affections. For example, in the toxæmiæ of scarlatina, renal disease, narcosis, alcoholism, fever, gout, and rheumatism, no appreciable change may be observed after death, and no proof can be adduced that pressure on the brain was exerted during life. Although the nervous centres present no lesion, or evidence sufficient to account for death, it cannot be with propriety referred to a primary neurosis of the brain, or simple apoplexy, but to a profound, probably structural, nervous change, though it may be inappreciable to ordinary methods of investigation. Further, in presence of pressure, symptoms of unconsciousness may be wanting, as in cases of intracranial tumour and aneurism, and even in *sudden* depression of the cranial vault. Again, limited extravasation into the substance of the brain, subsequently demonstrable, when death occurs at a more or less remote period, may produce a merely temporary unconsciousness (? shock), although the source of pressure cannot be removed until a more extended lapse of time. In ramollissement, I am not aware that the coma which may ensue is declared to be the result of pressure; and in cases of embolia, the insensibility is commonly referred to interference with the circulation.

Do not these seeming inconsistencies admit of solution by nutritional considerations, by impairment, perversion, and deficiency of the blood; by the various faulty, diseased, specifically altered, and poisoned states of this fluid, from morbid causes already categorically described?

At any rate, the most prejudiced supporter of vascular pressure cannot deny that his doctrines are *at the least* equivocal, and the feeble averment that turgidity of the vessels sufficient to produce, may disappear after, death, does not contribute to allay the scruples of the unbiassed investigator, who is, in justice, desirous to be informed of the *primum mobile* which can initiate and disperse a so conveniently evanescent congestion.

ART. II.

On the Diagnosis of the forms of Bright's Disease. By GRAINGER STEWART, M.D., F.R.C.P.E., F.R.S.E., Pathologist and Extra Physician to the Royal Infirmary, Lecturer on General Pathology, Surgeon's Hall, Edinburgh.

IN a paper which was published in the 'British and Foreign Medico-Chirurgical Review' for October, 1865, I described what I believe to be the true morbid anatomy of the diseases included under the name Morbus Brightii. In the present paper I propose to indicate the clinical history of each of these diseases, and to adduce a few cases illustrative at once of the symptoms and of the pathological conditions I have described.

The diseases are three in number, and each of them may be divided into different stages. They may best be shown in a tabular form, as follows:

I. Inflammatory form ...	*Stage* 1. Inflammation, which may be acute, chronic, or desquamative.
	„ 2. Fatty transformation (large, pale, fatty kidney).
	„ 3. Atrophy.
II. Waxy or amyloid form	*Stage* 1. Simple degeneration of vessels.
	„ 2. Degeneration of vessels with transudation into the tubules (large white kidney).
	„ 3. Atrophy.
III. Contracting cirrhotic or gouty form.	

It is to Virchow that we are indebted for clearly distinguishing the three forms of the disease, one the inflammatory, affecting primarily the tubules; another the waxy or amyloïd, affecting primarily the vessels; and the third the contracting or cirrhotic, affecting primarily the fibrous stroma of the organ. To these forms he applied the names Parenchymatous Nephritis, Amyloïd Degeneration, and Interstitial Nephritis.[1] Few pathologists would now be inclined to maintain that the first form is always parenchymatous, that the second is correctly described as amyloïd, or that the third is always a result of inflammation. On this account, I think it better to employ the simpler terms inflammatory; waxy or amyloid; and contracting, cirrhotic, or gouty kidneys.

On the other hand, it was early surmised by Bright[2] and Christison[3] that at least some of the various forms of diseased kidney with which they met might be examples of the same disease, in different stages, but it was not until Frerichs wrote that the stages of any of the forms were at all clearly defined. He traces the stages of the inflammatory form only, but these he describes exceedingly well. He says, in his excellent work,[1] "We divide the anatomical changes in the kidney in morbus Brightii into three forms, which at the same time may be regarded as stages of a common process:—1. The stage of hyperæmia, and commencing exudation. 2. The stage of exudation, and commencing transformation of the exudation. 3. The stage of retrogression or atrophy." To him, then, it appears that we are indebted for clearly laying down the series of changes which occur in the inflammatory form of the disease.

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[1] 'Cellular Pathology.' Translated by Dr. Chance, p. 381.

[2] 'Reports of Medical Cases,' by Richard Bright, p. 69.

[3] 'On Granular Degeneration of the Kidneys,' by R. Christison, p. 4.

In regard, however, to a subject which has been so widely studied, it is very difficult to trace back all the points to their original discoverers; suffice it to say that, in addition to those already mentioned, good service has been done by Rayer,[2] Johnson,[3] Gairdner,[4] Todd,[5] Garrod,[6] Goodfellow,[7] Dickinson,[8] Rosenstein,[9] Bennett,[10] and others.

I must, before passing on to the proper subject of this paper, say a few words with regard to two other forms of Bright's disease which are described by some authors, viz. "catarrhal nephritis" and "nephritis from passive congestion." First, with regard to the catarrhal nephritis. Let us, to begin with, understand the class of cases to which the name is applied. The description which Rosenstein gives is briefly the following:—The kidney is of normal size, or slightly swollen; in the more severe cases congested, and with hæmorrhages and ecchymoses scattered throughout the substance. The process begins at the apices of the pyramids, which are at first congested, afterwards pale. The cells are increased in number and in size, and the pallor in the latter stages depends upon their accumulation in the tubules. The glomeruli and the parts of the tubules next them are often natural; the stroma in many cases somewhat increased in volume.

The symptoms of this affection are by no means prominent, consisting merely of the occurrence of albumen, mucus, and casts in the otherwise natural urine. The casts are epithelial, with the so-called fibrinous cylinders, containing sometimes blood-corpuscles, pigment-granules, or oxalates. Some cases of this kind occurring in otherwise healthy individuals, the urine not being examined, are not recognised as renal affections at all, but are passed over as slight colds, rheumatic attacks, or gastric derangements; but there exists another and more common class of cases in which the renal affection is distinctly secondary to other important diseases. Among the causes of these secondary affections, Rosenstein ranks first *catarrhal conditions of the mucous membrane of other parts of the urinary tracts, the ureter, bladder, or urethra,* from which inflammatory action spreads by continuity of tissue. Such cases often linger long after the irritating cause has disappeared. Next among the causes he ranks *the action of substances which specially irritate the kidneys,* as cantharides, cubebs, copaiba. The third great cause is the *typhus fever poison,* which frequently leads to catarrhal nephritis. In a great epidemic which Rosenstein witnessed in 1857 the majority of the cases had temporary albuminuria, and that without any increased danger. The fourth great cause of catarrhal nephritis is the *cholera poison.* In that affection the essential changes are—hyperæmia, destruction and throwing off of the epithelium, without any alteration of the vascular apparatus. The fatty transformation of the epithelium may be very rapid, indeed, Johnson and Buhl found it within seventeen hours of the commencement of the attack.

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[1] 'Die Bright'sche Nierenkrankheit und deren Behandlung;' Von D. F. T. Frerichs, Braunschweig, 1851, p. 20.

[2] Rayer, 'Maladies des Reins,' 1839.

[3] Johnson, 'Diseases of the Kidneys.'

[4] Gairdner, 'Contributions to the Pathology of the Kidney,' 1848.

[5] Todd, 'Clinical Lectures on Urinary Diseases.'

[6] Garrod on 'Gout.'

[7] Goodfellow, 'Diseases of the Kidneys and Dropsy,' 1860.

[8] Dickinson, 'Med.-Chir. Transactions of London,' vols. xliii, xliv.

[9] Rosenstein, 'Die Pathologie und Therapie der Nierenkrankheiten,' Berlin, 1863.

[10] Bennett, 'Principles and Practice of Medicine.'

Such is the outline of the disease, which some observers regard as a perfectly distinct affection of the kidneys. But it appears to me that the catarrhal form really constitutes, and should be reckoned as, a variety of the inflammatory affection. In the kidney, as well as in other organs with whose morbid changes we are acquainted, pathological conditions constitute the best basis for classification. In all the varieties of catarrhal nephritis essentially the same condition exists as in the ordinary inflammatory affections, viz., a swelling and infiltration of the epithelium of the tubules. It is true that the amount of the organs affected, and the severity of the inflammation, varies in the different cases, and that in the catarrhal the amount is but slight. Still, the change is essentially the same, and the difference in degree accounts for all the other differences in the symptoms and in ultimate results. It is further to be observed that most of the causes which lead to catarrhal nephritis may induce true inflammatory disease if intensely or continuously applied, as Rosenstein himself distinctly admits in the case of typhus, for he remarks that true diffuse nephritis is not uncommonly seen as a result of the typhus poison, which usually induces merely catarrhal nephritis.

In respect both of the anatomical characters and of the causes which induce them, the whole of the inflammatory affections of the kidneys should be classed together. The anatomical characters are essentially the following:—1. That the organ is more or less swollen, and usually flabby, particularly in its cortical substance. 2. That there is more or less marked congestion of the surface and the substance of the organ. 3. That the epithelium of the tubules is swollen, opaque, granular, or undergoing fatty transformation. 4. That in many cases dark matter (free exudation) or blood is poured out into the lumen of the tubule.

The conditions with which I have seen these lesions associated are the following:—1. Exposure to cold, as in ordinary simple inflammation. 2. Scarlatina, diphtheria, erysipelas, which doubtless depend upon blood poisons. 3. Pyæmia, in its different forms. 4. Acute yellow atrophy of the liver. 5. Typhus fever. 6. Smallpox. 7. Diabetes mellitus. 8. Pneumonia.

Some of these affections are universally recognised as causes of acute nephritis; others are not, but it seems to me very clear that if we admit one of the series we must admit all.

With regard to the form of renal disease which results from passive congestion, I think that in the great majority of cases the kidney is not inflamed, but the symptoms depend entirely upon backward pressure on the blood; that in other cases a simple induration of the organ occurs, which cannot be held to be a form of Bright's disease at all; while in a very small proportion of instances a true inflammatory action coexists with and is doubtless aggravated by, but does not depend upon, the cardiac or vascular obstruction.

Having said so much with regard to the pathological anatomy, I now pass to the proper subject of this paper.

I. THE CLINICAL HISTORY OF THE INFLAMMATORY FORM OF THE DISEASE.

After exposure to cold and wet, or after, or, it may be, during an attack of scarlatina, erysipelas, or other febrile affection, or without apparent cause, an individual feels some lumbar pain; his urine, diminished in quantity, is of a dark or bloody colour, or presents a smoky appearance, contains a large amount of albumen, and throws down a precipitate composed mostly of tube-casts, bloody, epithelial, or finely granular. The face, the legs, or the scrotum become œdematous, and that œdema more or less rapidly increases and extends. These symptoms continue for a few days, or for a week or two, and then gradually subside, the urine first becoming paler, less albuminous, more copious, and the dropsy passing away by degrees. This constitutes the most favorable termination of a case, resulting in complete recovery.

But in a certain proportion of cases the symptoms increase in intensity, the dropsy becomes so severe as to prevent the free play of the lungs, and death from suffocation results; or the blood becomes poisoned with excrementitious matter, which the diseased kidneys are unable to eliminate, a series of nervous symptoms varying in their character, but at present grouped under the name uræmic, ensues, and so the patient dies. In a considerable proportion of cases the patient dies of the blood poison which has produced the renal affection, even before dropsy has appeared. In all the cases which prove fatal in the early stage the organs are found more or less congested, the cortical substance often pale, the renal cells swollen and granular, sometimes free exudation occupying the lumen of the tubes; in fact, the kidneys present the well-known characters of acute nephritis.

Between these two terminations of acute nephritis there is a third, by no means uncommon, in which the quantity of urine rises, its general quality improves, it becomes paler, less albuminous, and entirely free from blood. It still deposits tube-casts, but their quantity is diminished, and their character is changed. They still contain cells which are no longer finely granular, but loaded with fat, and in many cases so numerous as to give the whole cast the appearance of being composed of fatty granules; whilst others are hyaline, with fat-cells imbedded in them, and others simply hyaline in appearance. At the same time the dropsy does not diminish, or alternately rises and falls. The patient lingers on thus for weeks, or even a month or two. The respiration gradually becomes more embarrassed from the extension of dropsy to the lungs, and a fatal result follows, either from this cause or from uræmia. If examined in this stage, the kidneys are found to be large, pale, and fatty—in the second stage of the disease, commonly called the large fatty kidney.

Instead of this result, the dropsy may gradually disappear. The amount of albumen may greatly diminish, that of urea, &c., rise to the natural standard, and the patient may return to work, presenting no unfavorable symptom excepting albuminuria. Nay, even that may disappear, and the health may be entirely restored.

But in other cases no such favorable termination is reached; the urine may be pale, of good quantity, containing a moderate amount of albumen, and throwing down a slight deposit of tube-casts mostly hyaline, or only a few tube-casts may be found on the most careful examination, but the dropsy never wholly disappears, the patient cannot return to work, or if he does is at once thrown back, and sooner or later his symptoms become aggravated, and he dies with increase of dropsy or with uræmia, almost constantly with diminution of urine.

If the kidneys be examined at such a period as this they are found in the third stage of the disease—that of atrophy. The time required for the development of this condition probably varies considerably. I have never met with it when the disease was of less than five months' standing, but a large majority of the patients in whom I found it had been ill for more than a year.

In illustration of this outline I shall give the histories of a few cases.

CASES OF THE INFLAMMATORY FORM FATAL IN THE FIRST STAGE.

CASE 1.—*Acute nephritis following erysipelas, fatal on the second day, comolicated with pneumonia, pericarditis, &c.*—Mrs. B—, æt. 44, was admitted to the Royal Infirmary, under the care of Prof. Bennett, on November 21st, 1865, for eczema. On the 7th of January, when nearly recovered from the eczema, she became affected with erysipelas; she had distinct head symptoms. On the 12th her urine became albuminous,and she died on the 13th.

Autopsy, twenty-four hours after death.—The scalp was inflamed and thickened, infiltrated with serum. The skull-cap was of natural thickness. The membranes of the brain were congested. There was some serous effusion in the subarachnoid space. The substance of the brain was congested and œdematous. There were slight traces of pericarditis, a little lymph on the right auricle. The lower part of the right lung was in a state of red hepatization. Near the apex there were several masses of old tubercle. The left lung was natural. There were some pale thickened patches on the capsule of Glisson. The substance of the liver was natural. The spleen was natural. There were a few tubercular ulcers in the intestines. The kidneys were of natural size, not congested. The cortical substance had a peculiar homogeneous appearance. The surface was smooth.

On microscopic examination no free exudation was seen in any tubules, but the epithelium was in many parts granular and swollen. The Malpighian bodies were peculiarly dark, and their epithelium was cloudy and granular. No desquamation of the epithelium at any part could be detected.

Commentary.—The patient evidently died of the results of a blood poison, which affected the lungs, the brain, and the kidneys, and which may probably have either resulted from or caused the erysipelas. It presents an example of a very early stage of the disease, the albumen having appeared in the urine only a few hours before death.

CASE 2.—*Acute nephritis, complicated with peritonitis, fatal in an early stage; no dropsy.*—H. J—, æt. 17, was admitted to the Royal Infirmary under the care of Dr. Sanders, December 12th, 1865.

The patient was a prostitute, and had begun to menstruate about ten days before her death. She had some vomiting and purging, which came on speedily after a severe fright. She was admitted to he hospital comatose, and quite free from dropsy. She died thirty-six hours after admission. During that time she passed a small quantity of urine, which contained albumen, and deposited a copious precipitate of granular epithelium-tube-casts.

Autopsy.—The body was well nourished; the heart was natural; the lungs were congested, and the bronchi contained fluid. There was general acute peritonitis, the folds of intestine matted together by recent lymph, and coated with pus. The liver was somewhat pale; the outline of its lobules distinct. There was some fatty degeneration of the peripheme of the lobules. The spleen was natural. The mucous membrane of the intestines was natural. There was much congestion about the Fallopian tubes and ovaries, and it appeared probable that the peritonitis had resulted from the escape of the contents of a Graafian vesicle. The kidneys were slightly enlarged, in some parts congested; their cortical substance, of an ash-gray colour. The epithelium in all the tubules was swollen, cloudy, and granular, undergoing a rapid fatty degeneration. Numerous casts were easily scraped from the surface, but the weight of a covering glass sufficed to break them down into a granular débris.

Commentary.—In this case the renal disease may have been secondary to the peritonitis, or, perhaps, they may both have been manifestations of one poison. The death, at all events, occurred in an early stage of the inflammatory form of Bright's disease.

CASE 3.—*Acute nephritis consequent upon scarlatina, fatal in an early stage; no dropsy.*—J. A—, æt. 10, had been healthy until November 15th, 1862. He then had rigors. A scarlatinal rash appeared on the 15th. He was admitted to the Royal Infirmary, under the care of Prof. Bennett, November 16th. A trace of albumen was then present in the urine, which was secreted in natural amount. The albumen afterwards disappeared. Head symptoms, mainly delirium, became severe, accompanied by inflammation of the lymphatic glands of the neck. The patient died on the fifteenth day of the fever.

Autopsy.—The body was not dropsical. The glands of the right side of the neck were enlarged and suppurating. The tonsils were ulcerated. In the trachea there was an ulcerated patch of the size of a florin. The lungs were congested, and somewhat œdematous. The heart was natural. The liver and intestines were normal. The kidneys were enlarged, congested, the tubules contained exudation. In the ventricles of brain and subarachnoid space there was an effusion of clear serum.

Commentary.—In this case we have an example of the renal affection consequent on scarlatina, but there was a complete absence of dropsy, the patient having died before that symptom became developed.

CASE OF THE INFLAMMATORY FORM RECOVERING AFTER FIRST STAGE.

CASE 4.—*Acute nephritis; recovery.*—A. D—, a baker, was admitted to the Royal Infirmary, under the care of Dr. Laycock, November 5th, 1859.

Had always been healthy until October 18th, when he caught a severe cold. On November 1st he observed his face and afterwards his body and extremities swell. The œdema gradually increased until the date of admission.

He then was intensely œdematous. His urine was scanty, about two ounces in the day, of a pale straw colour, highly albuminous, with a considerable sediment, consisting of granular tube-casts.

Under treatment by dry cupping, diuretics, &c., he soon improved. The urine increased in quantity, the albumen diminished, and the dropsy gradually disappeared. He was dismissed apparently quite well November 25th.

CASES OF INFLAMMATORY FORM FATAL IN THE SECOND STAGE.

CASE 5.—*Acute nephritis following upon exposure to cold; dropsy; fatal in second stage.*—B. M—, a servant, æt. 17, unmarried, was admitted to the Royal Infirmary, under the care of Professors Laycock and Bennett, January 20th, 1860.

Previous history.—Had always been healthy until fifteen months before admission, when, after having had her feet wet, she observed them to be swollen. This gradually subsided, but she continued subject to occasional palpitation, increased by exertion. On January 6th, a fortnight before admission, her stockings got very wet, but she continued to wear them during the rest of the day. Next morning her face and legs were swollen, and she felt considerable palpitation. These symptoms did not diminish. She sought admission to the Royal Infirmary.

On admission.—The face and legs were œdematous, and she appeared anæmic. The heart sounds were natural. There was some embarrassment of respiration, and sibilant and snoring râles were heard over the chest. There was some dulness towards the bases. The digestive and nervous systems were normal. There was considerable tenderness on pressure over the kidneys. The urine was small in quantity, muddy, and deposited a precipitate composed of blood-corpuscles and tube-casts, some bloody and some granular. It was highly albuminous. She was treated by diuretics, by dry and wet cupping over the kidneys, by various other plans, without material benefit. The dropsy gradually increased, also the embarrassment of breathing. The amount of urine increased; the blood disappeared from it. Casts with fatty epithelium replaced the granular and bloody casts formerly so abundant, and among them not a few hyaline casts were found. Sloughing sores formed on her legs, and she died with symptoms of pyæmia, April 14th.

Post-mortem examination.—Only the kidneys were examined. They were enlarged; the surface was smooth, mottled with yellow, fatty, sebaceous-looking matter; the capsule easily peeled off. On section the cortical substance was found enlarged, dense, opaque, and fatty, and on microscopic examination the tubules of the cortex and some of those of the cones were found distended with exudation and fatty matter. There was no change in the vessels or stroma.

Commentary.—This case lasted for three months and a week, and the second stage of the renal affection had been reached. There was not a trace of atrophy. The whole of the cortical substance of the kidneys was shut off from functional activity, and unless some means had been successful in removing the exuded material recovery was almost impossible.

CASE 6.—*Nephritis fatal in second stage.*—E. C—, æt. 48, a washerwoman, was admitted to the Royal Infirmary, under the care of Dr. Bennett, March 14th, 1860.

History.—She had always been healthy until October, 1859, when she got her feet wet during a menstrual period. She then felt cold and shivering, but continued to work during the day; at night she had distinct rigors, and she observed that the quantity of her urine diminished, and that it was of a dark colour. She continued to work with the aid of stimulants for three weeks, but at the end of that time dropsy of her feet and ankles compelled her to take rest. The swelling steadily increased. In the beginning of February she felt some difficulty of breathing and complained of a cough. All these symptoms increasing, she sought admission to the infirmary.

On admission.—There was general anasarca; the abdomen was much distended, measuring forty-four inches in circumference. There was no pain on pressure over the kidneys. The urine was in small quantity, smoky, of sp. gr. 1010, very albuminous, and threw down a thick whitish sediment, composed of granular and fatty tube-casts, with some blood-corpuscles. The tongue was furred, the appetite bad, the bowels constipated. There was much dyspnœa, considerable consolidation of the left lung posteriorly. The cough was frequent and accompanied by a tenacious purulent expectoration. The heart was natural.

After admission.—Under powerful diuretics the quantity of urine rose, but again gradually subsided, notwithstanding the continuance of the drugs. Bitartrate of potash seemed to be the most useful, but appeared to increase the quantity of blood discharged by the kidneys. The dropsy never materially diminished, and the dyspnœa was constantly increasing. She died exhausted, on April 7th.

Post-mortem examination, thirty hours after death.—There was general anasarca, and grea tascites. There was also effusion into the pleuræ, and a considerable amount of mucus in the bronchi. The heart was natural. The liver was congested, weighed three pounds. The spleen was small and dense, weighed two ounces. The intestines were healthy. The kidneys were enlarged, pale, and mottled; they weighed together fifteen and a half ounces. The capsule was easily stripped off. The cortical substance was swollen, opaque, and fatty. The tubules were found on microscopic examination distended with exudation, the epithelium to a great extent fatty.

Commentary.—This was a less acute case than the last. It proved fatal in the second stage, and had existed for six months.

CASE 7.—*Nephritis following upon cold, fatal in second stage; uræmia, dropsy, apoplexy.*—J. P—, æt. 40, was admitted to the infirmary, under the care of Dr. Laycock, February 15th, 1865. Was a married woman, of temperate habits. About the middle of November, 1864, she was exposed to cold and wet. She then became affected with lumbar pain, swelling of the face, afterwards of the legs, and ultimately of the whole body. The urine was high-coloured and diminished in quantity.

On admission.—She had bronchitis. The whole body was œdematous. Her urine contained much albumen and a large number of granular tube-casts. She did not improve under treatment, but gradually became worse until the middle of April, when she had convulsions. She then became comatose, and died April 16th.

Autopsy.—The body was very œdematous. The peritoneum was distended, and there was a considerable amount of fluid in the pleuræ and pericardium. The heart was natural. The lungs were congested and œdematous. The liver was small, congested, and fatty. The spleen was natural. The kidneys were enlarged, and presented the characters of a typical example of the large fatty kidney, the second stage of the inflammatory form of Bright's disease. There was some serous effusion into the subarachnoid spaoe, and in the optic thalami there were recent clots.

Commentary.—This case was an example of the inflammatory form, terminating during the second stage, five months after the commencement of the malady.

CASE OF PARTIAL RECOVERY DURING THE SECOND STAGE OF THE DISEASE.

CASE 8.—*Acute nephritis; partial recovery during the second stage; extreme dropsy, &c.*—R. P—, æt. 26, a maltster, had been healthy until January, 1865. He had been originally a baker by trade; he had four years before become maltster; in both businesses was much exposed to vicissitudes of heat and cold.

About the end of January, 1865, he noticed his feet swelling. The œdema increased rather rapidly. He had frequent calls to micturition, but he made little water at a time, and that dark and bloody. He was admitted to the infirmary on March 3rd. Under diuretics the urine increased in quantity and he somewhat improved, but the dropsy again increased and the urine diminished. When he came under my observation, on April 27th, the following was his condition:—The face was pale and pasty. There was great dropsy of the legs and feet, scrotum, penis, and flanks, and considerable ascites. The urine was pale, but smoky, of sp. gr. 1020, highly albuminous, and deposited a precipitate containing numerous casts, hyaline and fatty; the quantity was forty ounces. There was a good deal of bronchitis. Hot-air baths and acetate of potash had been tried for some time. He was ordered, April 28th, a dessert-spoonful of Infusum Digitalis three times a day, and on the following day a drachm of Elect. Bitart. Potass. was added. These did not induce diuresis, indeed the urine rather diminished in amount and the dropsy increased. May 3rd, he was ordered to inhale forty minims of Ol. Junip. twice a day. The quantity of urine at once increased, and the dropsy began to diminish. The quantity of water steadily increased, rising from forty-six ounces on May 1st to eighty-four on the 12th, and afterwards to more than one hundred ounces. On May 11th a few pricks with a needle were made in the left leg. A considerable amount of water drained away. On the 24th dropsy had disappeared.

From this time he pretty steadily improved. The dropsy quite gone. The urine in good quantity, with a natural amount of urea and salts, and the albumen diminished. The fatty were replaced by hyaline casts. He was dismissed on August 8th, quite well, excepting that his urine was albuminous.

In October he was exposed to cold and damp in his situation as light porter, and in consequence had a reaccession of his disease. The quantity of urine was reduced to twenty ounces. It was very albuminous, with tube-casts containing fatty cells. The dropsy was very considerable. Under diuretics he again steadily improved, and was again dismissed on December 15th. The albumen continued at date of dismissal.

Commentary.—The case was a typical one of acute nephritis, passing through the second stage of fatty degeneration, and becoming chronic. The value of diuretics was very apparent, and particularly of the oil of juniper, given as inhalation, as first recommended by Sir James Simpson. A distinct increase of the flow of urine followed its administration, and from my experience of it in this and other cases, I think it is a remedy which should be tried in all obstinate cases of dropsy.

CASES OF INFLAMMATORY FORM FATAL IN THIRD STAGE.

Case 9.—*Nephritis, fatal in third stage; general dropsy, &c.*—E. B—, a female, æt. 62, was admitted to the Royal Infirmary, under the care of Dr. Haldane, in October, 1865. She was then extremely anasarcous, passed small quantities of albuminous urine. Notwithstanding the use of powerful diuretics, the amount of urine continued low, the dropsy became more intense, and she died October 13th. She stated that she had been healthy until eight months before, and had then become dropsical.

Autopsy.—The body was very œdematous; the abdomen was distended with clear serum. The pleuræ and pericardium also contained much fluid. The lungs were compressed. In both apices there were traces of old tubercle. The heart was enlarged; the left ventricle was much hypertrophied; the valves were competent; the aorta was atheromatous, in part calcareous. The liver weighed one pound twelve ounces, its substance was congested. The spleen was small. The kidneys weighed together nine ounces. Their surface was granular. On section their cortical substance was found to be infiltrated with fatty sebaceous-looking matter, and in a state of commencing atrophy. On microscopic examination many of the tubules were found to be fatty and many in a comparatively natural state. The intestines were very œdematous.

Commentary.—This case had lasted about eight months, and the third stage of the disease had been reached. The hypertrophy of the left side of the heart had resulted partly from the atheroma of the vessels, and partly from the renal affection. The death was due mainly to the intense dropsy, which was in part referable to the disease of the circulatory system, in part to the renal affection.

Case 10.—*Nephritis, fatal in third stage; general dropsy, &c.*—E. T—, æt. 30, was admitted to the Royal Infirmary, under the care of Dr. Haldane, April 3rd, 1865. In summer, 1864, she had caught cold, in consequence of which her urine diminished in quantity and became high-coloured. She then also had dropsy. She never fully recovered. While under treatment she had dropsy to a considerable extent, and sank exhausted.

Autopsy.—The body was œdematous. The pleuræ, pericardium, and peritoneum contained clear serum. The heart was enlarged and dilated. The left side was much hypertrophied. The right side was dilated. The valves were free from disease. The lungs were partially collapsed, from the pressure of the dropsical fluid, but their margins were emphysematous. The liver weighed two pounds six ounces. Its capsule was thickened. The spleen was natural. The kidneys weighed together seven ounces. Their surface was granular; the capsule peeled off readily. On section the cortical substance was found pale, fatty, and partially atrophied. Many of the tubules contained fatty matter within the epithelial cells.

Commentary.—The case afforded an excellent example of this form of disease in an advanced stage. The organs were considerably reduced in size. The marked hypertrophy of the heart appeared to depend mainly upon the renal affection. The disease had lasted fully a year.

CASE 11.—*Nephritis, repeated acute attacks, fatal in the third stage, with uræmia and general dropsy.*—P. M—, æt. about 30, had been for upwards of a year under Dr. Haldane's observation in the Royal Infirmary. Early in 1864 he had acute nephritis, from which he gradually recovered. He then had repeated acute exacerbations, and ultimately came into the hospital with severe general dropsy; diminished flow of urine, and that very albuminous, and containing many fatty and hyaline casts. The quantity of urine did not increase, uræmic convulsions came on, and death ensued on January 5th, 1865.

Autopsy.—There was general anasarca, and dropsy of the serous cavities. The lungs were œdematous, contained some carbonaceous deposit. The bronchi were congested. The heart was enlarged. The left ventricle was much hypertrophied, the right dilated. There was no valvular disease. Both the liver and spleen were congested. The intestines were congested, otherwise natural. The kidneys were of about the natural size, their surface was granular, mottled, and opaque. The capsule was somewhat adherent. The cortical substance nnd relatively somewhat diminished in size. On microscopic examination many of the tubules were found full of dark fatty matter, while some had natural epithelium. The brain was somewhat œdematous, the arteries at its base atheromatous.

Commentary.—In this case the disease had lasted fully a year, and the third stage had been fully established. The hypertrophy of the heart was in part due to the renal affection, and in part to the disease of the arteries.

CASE 12.—*Nephritis, fatal in third stage; general dropsy.*—J. G—, æt. 66, was admitted to the Royal Infirmary November 18th, 1865. She had been in general healthy, though of intemperate habits, until the end of 1863, when she had dropsy and lumbar pain. In October, 1865, she got wet, and dropsy recurred more intensely than before.

On admission.—There was considerable anasarca and œdema of lungs and general bronchitis. The urine was of a pale straw colour, of sp. gr. 1010. It contained some albumen, with fatty and hyaline casts. During December its quantity varied from 32 to 100 oz.;

in January from 40 to 70 oz. During these months she was taking powerful diuretics. The dropsy went on increasing, and she died February 11th.

Autopsy.—The body, particularly the face, arms, and chest, was extremely anasarcous, and there was considerable effusion in the serous cavities. She was not emaciated. The heart was enlarged, hypertrophied, and dilated. The aortic valves were competent; there was some fibrinous thickening of the margin of the mitral and some parts of the aortic. There was no atheroma of vessels. The lungs were compressed, particularly the left. Both were œdematous. The liver was congested. The spleen was small. The kidneys weighed together 10 oz.; their surface was granular, but the capsule peeled off easily. On section the cortical substance was of about its natural size, but pale, fatty, full of dense sebaceous-looking matter. The stomach and intestines were natural. The brain was somewhat œdematous, and there was some subarachnoid effusion.

Commentary.—We have here a disease of the kidneys of upwards of two years' duration. From the increase in the amount of urine which took place during the first months that she was under treatment, it appears that but for the serious affection of the heart she might have recovered.

II. CLINICAL HISTORY OF THE WAXY OR AMYLOÏD DEGENERATION.

Several years ago the following outline of the symptoms of the waxy degeneration was published in the 'Edinburgh Medical Journal:'[1]

"An individual who has long suffered from wasting disease, such as scrofula, caries, necrosis, or syphilis, or who, though without palpable disease, is of a feeble constitution, feels an increasing weakness, and begins to pass large quantities of urine, and to drink largely. He is, contrary to his usual custom, obliged to rise repeatedly during the night to make water, and on each occasion passes a considerable quantity. The amount of urine varies from 50 to upwards of 200 oz. daily, always bearing a relation to the amount of fluid drank, generally nearly equalling it in amount, or sometimes even exceeding it. The feet and ankles become œdematous after a hard day's work, but return to their natural condition during the night's repose. In many cases there is observed a hardness and swelling in the hepatic and splenic regions, dependent on an increase of bulk of the liver and spleen. The patient feels a constant lassitude and unfitness for exertion. His urine gradually becomes albuminous, and a few hyaline tube-casts are to be found in the very scanty sediment which it throws down. It is of low sp. gr.—1005 to 1015. The blood in many cases presents some peculiarities when examined microscopically, the white corpuscles being somewhat increased in number, and the red presenting a flabby appearance, with a marked tendency to tail—that is to say, instead of forming into rouleaux, like healthy corpuscles, they become stretched out into long spindle-shaped bodies. These changes I have observed only when the degeneration affected the lymphatic or blood-glands. The patient may continue in this state for months, or even years—may, indeed, undergo a temporary improvement—the liver and spleen becoming diminished in bulk, and the blood resuming a more healthy character; but, sooner or later, for the most part ascites or general dropsy gradually supervenes, accompanied frequently by diarrhœa, which is at times found quite uncontrollable. The urine, now very albuminous, diminishes in quantity, so as at times to be almost or altogether suppressed; effusions into the serous cavities or severe bronchitis ensues; the patient becomes exhausted, and sinks, or drowsiness comes on, and the disease terminates amid coma and convulsions."

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[1] 'Edinburgh Medical Journal,' Feb., 1861, also for Aug., 1864.

Such was the description of the clinical history of the disease which I gave in 1861, and my experience has confirmed the views I then advanced, particularly in regard to the early history of the cases. I find that while the termination I then described—that is, with the supervention of an inflammatory affection—a disease of the tubules is not infrequent, occurring in more than one third of the cases, very many cases prove fatal without this complication, either from disease elsewhere or from the degeneration itself. I subjoin a few illustrative cases.

CASES OF THE WAXY FORM FATAL IN FIRST STAGE.

Case 13.—*Waxy degeneration of kidneys, spleen, intestines, and liver; tubercle of lungs, lumbar abscess, &c.*—J. C—, æt. 14, a deformed boy, was admitted under my care in the Royal Infirmary in January, 1866, on account of lumbar and psoas abscess, with tubercle of the lungs. He passed from seventy to one hundred or more ounces of urine daily. At first it contained no albumen; afterwards slight traces were found, although by no means constantly. In April he became affected with exhausting diarrhœa, at times dysenteric in its character; and in consequence of this and the wasting discharge from the abscess he died exhausted on May 7th. He never had a trace of dropsy.

Autopsy.—The body was much emaciated and deformed. There was an opening in the abdominal parietes close to Poupart's ligament, and another in the lumbar region of the right side. The abscess was connected with caries of the bodies of several of the dorsal vertebræ. The lungs contained some obsolete tubercles. In the mitral valve there were some vegetations. The liver was large, fatty, and waxy. The spleen was bound down by numerous adhesions; its Malpighian bodies and small arteries were waxy. In the former there were numerous extravasations of blood. The intestines were waxy. The kidneys were small—in some parts atrophied. In some of the tubules there was slight evidence of inflammation; in many there was a fine fatty condition of the cells, but there was no relation between these and the distinctly waxy vessels. Both those of the cones and the cortical substance—small arteries and Malpighian bodies—had undergone the degeneration.

Commentary.—In this case we had the waxy degeneration originating in one of its most common causes—a wasting discharge from a carious bone, and manifesting itself (so far as the kidneys were concerned) by only one symptom, viz., the increased flow of urine; but so reliable do I consider the symptom that I ventured to anticipate on his first admission to the hospital the appearance of albuminuria, an anticipation which was not disappointed. The puckerings on the surface of the organs were, I think, results of slight local inflammatory action. There was no appearance of the disease advancing to the second stage—that of the large white kidney.

CASE. 14.—*Waxy degeneration of kidneys, tubercle of lungs and intestine, constitutional syphilis.*—J. M—, æt. 24, a man of syphilitic constitution, was admitted to the Royal Infirmary, under the care of Dr. Scoresby Jackson, with symptoms of incipient phthisis on March 8th, 1865. His tubercular disease gradually became worse, and became associated with colliquative diarrhœa. He died exhausted July 7th.

Autopsy.—The body was emaciated; the heart thin and feeble, the valves natural. Both lungs contained tubercular deposit, and were riddled with cavities. The liver weighed 4 lb. 6 oz., was waxy and fatty. On its surface there were a number of small cicatrices and adhesions to neighbouring parts. The spleen was waxy. In the intestines, particularly in the sigmoid flexure of the colon, there were numerous tubercular ulcers. The kidneys were somewhat pale; they weighed together 10½ ozs. The vessels of the cones and some of those of the cortical substance were slightly waxy. The tubules were natural.

Commentary.—This case, occurring in a syphilitic individual affected with pulmonary and intestinal tubercle, proved fatal from those affections before the waxy disease of the kidneys was far advanced. I was unable to obtain any information as to the amount and quality of the urine, but it is probable that, from the severe diarrhœa, the quantity could not have been much increased.

CASE 15.—*Waxy degeneration of kidneys, tubercle of lungs, &c.* —A. M— was under treatment in the Royal Infirmary, under the care of Prof. Laycock, for tubercle of the lungs. She died exhausted. On post-mortem examination the body generally was emaciated, and there was slight œdema of the labia. The heart was slightly dilated. The pleural surfaces were adherent, and the lungs contained much tubercle. The liver was enlarged and waxy; weighed 5 lb. 7 oz. The spleen was waxy. The kidneys were of natural size; the tubules were natural, the vessels in a state of waxy degeneration. The villi and vessels of the small intestine were waxy.

Commentary.—In this case also the renal symptoms were not recognised during life, attention having been mainly directed to the pulmonary affection.

CASES OF THE WAXY FORM FATAL IN SECOND STAGE.

CASE 16.—*Waxy degeneration of kidneys, syphilis, diarrhœa, &c.*—A. C—, æt. 30, was admitted to the Royal Infirmary, under the care of Dr. Sanders, May 30th, 1864. She stated that she had enjoyed good health until within four weeks of her admission, but for some months before she had observed that she passed a larger quantity of urine than natural. She was obliged to get up several times during the night to micturate. She had a little dropsy, but it disappeared on the occurrence of diarrhœa, a few weeks before admission. Her urine was pale, of sp. gr. 1010, contained much albumen, always exceeded 60 oz. daily, although she was affected at the same time with severe diarrhœa. She had frequent vomiting, and gradually became exhausted, and died June 28th. Her family was strumous. There was no positive evidence of syphilis, but she had a cachectic appearance, and complained much of pain in her bones.

Autopsy.—The body was somewhat emaciated; the heart and lungs were natural. The bronchi contained much muco-purulent fluid. The liver was large, weighed 4 lb. 6 oz., was bound to the diaphragm by numerous old adhesions; it was fatty and waxy throughout; both the cells and vessels were waxy. The spleen weighed 1 lb. 1 oz.; was extremely waxy. Both kidneys were enlarged, the left weighed 9½ oz., the right 7½ oz. The vessels were extremely waxy, both in the cortical substance and in the cones. The basement membrane of the tubes was also in some parts waxy. The intestines were waxy.

Commentary.—This case proved fatal, during the second stage of the disease, not from affection of the kidneys, but from the severe diarrhœa, which resulted from the disease of the intestines. The disease which, as we have seen, is insidious in its progress, had come on at least six months before her death, but it is very probable that it had existed even before that time. Notwithstanding the severe diarrhœa, she passed an excessive quantity of urine during the whole time that she was under observation.

CASE 17.—*Waxy degeneration of kidneys fatal in second stage;*

struma.—M. M—, æt. 17, was admitted to the Royal Infirmary, under the care of Drs. Bennett and Laycock, March 3rd, 1860. She had had carious disease of the vertebræ. During February she had noticed that she was making more water than usual. Her urine was always albuminous, ranged from 50 to 120 oz. daily, and deposited hyaline casts, which contained here and there a fatty cell. In May she had diarrhœa, and at the same time dropsy; her urine diminished to 30 or 40 oz. daily. She passed more tube-casts, and in the casts there were more numerous cells than formerly.

Autopsy.—The body was somewhat dropsical. The liver and spleen were waxy. The kidneys were large and pale; exhibited the characters of the second stage of the waxy with some degree of the inflammatory form. There was no ulceration of the intestines.

Commentary.—This was a very typical case of the waxy disease, both in respect of previous history and of symptoms. The patient died four months after she had noticed the increased flow of urine, but she did not come under observation until the albumen had appeared.

CASES OF THE WAXY FORM FATAL IN THIRD STAGE.

CASE 18.—*Waxy degeneration of kidneys fatal in third stage; uræmia, &c.*—J. P—, æt. 50, a quarry-man, resident in Edinburgh, was admitted under my care May 12th, 1865. He denied having had syphilis, and stated that he had been in general healthy. For some time before his admission he noticed that his feet became swollen at night, that he had frequently to get out of bed in order to make water, and that he had great thirst.

On admission.—His legs were œdematous. His complexion was good; his cardiac sounds were natural, his arteries atheromatous. There was a slight arcus senilis. The red corpuscles of his blood were flabby. He had some bronchitis. His appetite was impaired. His bowels acted naturally, his liver and spleen were of normal size. The urine averaged about 120 oz., was pale, of sp. gr. 1008, distinctly albuminous. No casts were found. Under treatment he improved, and was dismissed June 5th.

On the 12th of that month he returned, feeling decidedly worse. He then had some muscular twitchings, without loss of consciousness. These were followed by a severe convulsion on the 14th, during which he became quite unconscious. In the afternoon he had another severe fit. He gradually became comatose, and died on the 22nd.

Post-mortem examination twenty-three hours after death.—The body was not dropsical nor much emaciated. The heart weighed 1 lb. 3 oz., was dilated and hypertrophied. There was considerable atheroma of the arteries. The lungs were congested, œdematous, and emphysematous; contained traces of old tuberculous or syphilitic deposit. The liver weighed 4 lbs. Many of its cells were fatty, and its smaller vessels were waxy. The spleen weighed 6 oz., was not waxy. The kidneys were in the third stage of waxy degeneration. The vessels and villi of the intestine were waxy, but the cavity contained some hardened fæces. In the substance of the brain, in the white matter of the posterior lobe of the right hemisphere, there was a recent clot of the size of a pea; the substance generally was congested and œdematous.

Commentary.—The case was evidently of considerable standing. Notwithstanding his denial of it, I incline to think that he had been the subject of syphilitic disease. The case affords an excellent example of the latency of the symptoms in many of these cases of waxy degeneration. The condition of the urine was eminently characteristic of the disease; and but for the indications it afforded, it would have been impossible for us to have diagnosed the existence of the affection. There was no characteristic appearance of the countenance, no enlargement of the liver, no diarrhœa, no certain history of old-standing disease by which we could have been led to the opinion.

Case 19.—*Waxy degeneration of kidneys fatal in third stage; syphilis, uræmia.*—J. N—, æt. 37, a miller, was admitted under my care on May 13th, 1865.

Previous history.—Twelve years before admission he had contracted constitutional syphilis, and for eight years had been out of health. He suffered from various constitutional symptoms, and in 1862 he found that he was obliged to rise frequently during the night to make water. The daily amount of urine passed was found at that time to be from 150 to 180 oz.

His symptoms had gradually become worse; he had cough and some difficulty of breathing, and once, in March, 1865, he had a fit, which lasted for three minutes, and during which he was unconscious and moved convulsively.

On admission.—The complexion was generally fair; the cheeks and nose were red from dilatation of small vessels. The tongue was clean, the appetite good. The liver measured 6¼ inches vertically in the mammillary line. Heart sounds were natural. Some of the smaller vessels were atheromatous. The white corpuscles of the blood were somewhat increased in number, the red were soft and flabby. The nervous system was natural. The amount of urine ranged from 90 to 150 oz. daily; it was pale, of sp. gr. 1010, albuminous, and contained some hyaline and granular casts. By careful measurement it was ascertained that the amount of fluid consumed daily was less than the urine passed.

Early in June he became affected with diarrhœa; he also became peculiarly irritable, had distressing dreams, and fancied he saw black objects, particularly a large black rat, flitting about the ward. He also complained of pain in the left forehead. He gradually became exhausted and more delirious; he had a good deal of twitching of the muscles, but no convulsion, and died June 14th.

Post-mortem examination, twenty-six hours after death.—The body was not dropsical nor emaciated. The pericardium contained a little fluid. The substance of the heart was pale and fatty. The valves were competent, but there was some deposit at the bases of some of them. The bronchi were congested and contained a good deal of mucus. Both lungs contained syphilitic deposits, partially softened.

The liver weighed 7 lb. 11 oz. It was connected by adhesions with neighbouring organs. It also contained some syphilitic and waxy masses, which evidently bore a relation to the cicatrices.[1] Many of the small vessels were waxy. Many of the cells were waxy and many fatty. The spleen was much enlarged, waxy, weighed 3 lb. 3 oz. It contained peculiar cicatrices and deposits of altered blood. The suprarenal bodies were waxy. The kidneys were large, their surface granular, the increase of bulk being from syphilitic deposits. The vessels of the cortical and conical parts were extremely waxy. Some of the tubules contained fat; the right testicle contained a syphilitic deposit. The intestine was waxy, and contained one or two ulcers. The brain was congested and œdematous, contained no syphilitic deposit, and its vessels were not found to be altered. The cord was in the same condition as the brain.

Commentary.—This was a typical case following upon syphilis. The increased flow of urine had been observed for three years, and all the symptoms of the disease were well marked. The peculiar variety of uræmia which preceded death was also very interesting.

CASE OF WAXY DISEASE OF LONG STANDING MUCH IMPROVED UNDER TREATMENT.

Case 20.—The following case, that of A. M—, a shoemaker, æt. 35, I have already published in the 'Edinburgh Medical Journal,' and have brought his history up to 1864.[2] He continues under observation, and is in much better health than he was when he first came under my observation. He has had syphilis; has never had dropsy, but has passed from 80 to 150 oz. of urine daily, and that urine almost constantly albuminous. The first appearance of albumen in the urine was carefully watched in 1860, and, after continuing for upwards of five years, it seems to be now gradually disappearing. He has during all that time exhibited the leading symptoms of this form of Bright's disease, and, should his recovery be ultimately perfected, I should feel warranted in asserting that even well-marked waxy degeneration of liver and kidneys may be recovered from.

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[1] See 'Brit. and For. Med.-Chir. Rev.,' Oct., 1864.

[2] 'Edin. Med. Journ.,' Feb., 1861, and Aug., 1864.

IV.—Clinical History of the Gouty or Contracting Form.

This is in the mean time somewhat imperfectly ascertained. The early symptoms are very slight. I have seen several cases in which there was no renal symptom whatever during life, and such cases are, I believe, not unfrequent. The quantity of urine is natural, sometimes it is said rather to surpass the average. Albumen and casts are commonly present, especially towards the end of the cases. Towards the termination the urine may become scanty or even suppressed, and the patient dies, in most cases, not of the renal, but of some other affection.

CASES OF CONTRACTING KIDNEY, DEATH OCCURRING FROM OTHER CAUSES IN EARLY STAGE.

Case 21.—*Contracting kidney, tubercle of lungs and intestines; no dropsy nor albuminuria.*—J. M—, æt. 65, was a gardener, and was admitted to the Royal Infirmary, under the care of Dr. Sanders, November 7th, 1865. He had always been a sober and generally a healthy man till the end of September, when he took cough and pain in the chest. The sputum was purulent. The urine was 40 oz. daily, acid, of sp. gr. 1020, contained no albumen nor any deposit. He had no dropsy. Towards the end of November he took diarrhœa, and from that time became worse. He died January 20th, 1866.

Autopsy.—The body was emaciated. The visceral layer of the pericardium was œdematous, its substance fatty. The valves were competent, but somewhat thickened. The aorta was atheromatous. Both lungs contained tubercle and tubercular cavities. The liver was fatty; the spleen was natural. There were some tubercular deposits and ulcers towards the lower end of the ileum. The kidneys were small and pale, their surface slightly granular. The cortical substance was somewhat diminished. The fibrous stroma was distinctly increased. A few of the tubules contained exudation and some a little pigment, but the majority were quite natural.

Commentary.—Here we have a distinct change in the structure of the kidneys without any urinary symptom, the quantity and quality of the fluid having been natural.

Case 22.—*Contracting kidney, cardiac dilatation, emphysema, general dropsy, albuminuria.*—T. R—, æt. 67, had been admitted to the Royal Infirmary, under the care of Dr. Sanders, with dropsy, dilatation of heart, bronchitis and emphysema, albuminuria and general dropsy. These symptoms gradually increased, and proved fatal July 20th.

Autopsy.—The heart was enlarged, particularly the left side. The aorta was somewhat atheromatous, the valves quite competent. The lungs were congested, their lower part almost pneumonic; they contained some patches of pulmonary apoplexy. The liver was congested and soft. The spleen was firm. The kidneys were congested, their surface granular, the cortical substance relatively diminished; many of the tubules contained exudation, others were natural. The connective tissue was increased. The intestines were natural.

Commentary.—In this case it is probable that the dropsy and the albuminuria to a large extent depended upon the disease of the heart and lungs, but the case afforded a distinct example of what is comparatively rare in Edinburgh—the contracting kidney.

CASE OF CONTRACTING KIDNEY FATAL IN ADVANCED STAGE.

CASE 23. — *Contracting kidney, gouty deposits in joints, pyæmia, &c.*—T. B—, æt. 42, was very frequently under treatment in the Royal Infirmary, under the care of the clinical professors, on account of gout. On January 8th, 1860, he had a severe attack, and re-entered the infirmary.

On admission.—Both hands were deformed with gouty concretions. The joints were thickened, and contained deposits; some of them were very tender on pressure. The heart sounds were natural, there was considerable bronchitis, and some dulness on percussion at the apices of both lungs. There was no pain on pressure over the kidneys. The urine was of a very pale amber colour, of sp. gr. 1011, contained much albumen and a few hyaline casts. The nervous system was irritable; the tongue was furred and dry; the appetite poor, thirst great; the bowels loose.

During the spring he had several severe attacks of gout, and towards the end of May he took pneumonia, of which he died. He had no convulsions, but during the last week of his life he was unconscious of pain, and *felt* better than he had been for years.

In the spring the quantity of urine was about 60 to 70 oz. daily. In the last month of his life it fell to from 12 to 36 oz. daily.

Autopsy.—The body was not œdematous. Most of the joints were distorted, and many contained gouty deposit. In the liver there were numerous secondary abscesses. The spleen contained traces of secondary abscesses. The kidneys were greatly diminished in size; the cortical substance of the right was considerably atrophied. The left was reduced to the size of the last phalanx of a forefinger, and consisted only of a single cone with a little cortical substance attached to it. The pelvis and ureter were of natural size. In both organs there was a marked increase of fibrous tissue, with contractions of Malpighian bodies and tubules, and a deposit on many parts of the connective tissue of urate of soda in the form of acicular crystals. Many tubules were seen quite free from exudation.

In this paper I have endeavoured to sketch and illustrate the clinical history of these important diseases; I hope to show in a future communication what are the most frequent complications of each variety.

I cannot conclude without expressing my deep obligation to my colleagues in the infirmary, to whom I am indebted for permission to publish many of the cases recorded in this paper.

ART. III.

Case of Excision of a part of the Spinal Accessory Nerve for Spasmodic Wry Neck. By CAMPBELL DE MORGAN, F.R.S., Surgeon to the Middlesex Hospital, Examiner in Surgery to the Royal College of Physicians.

William Bishop, æt. 32, a healthy labouring man, living in the country, was crushed down by the weight of a heavy ladder which he was attempting to lower. His neck was bent under it, but no particular injury appeared to have been done, and he paid small attention to the accident. This occurred in October, 1860. He does not seem to have noticed anything until nearly two months after, in December, when he became affected with twitchings in the neck. He thinks, however, that for some months before the accident there had at times been a tendency to jerking of the neck to one side.

These spasmodic attacks, at first occasional, rapidly became more powerful and continuous, so as, in the course of two or three weeks, to thoroughly unfit him for work. On the 29th January, 1861, he was admitted into the Middlesex Hospital.

His appearance was peculiar. There was an anxious worn look in the countenance, which at times changed to a sort of sardonic smile, from spasm of the facial muscles. The eyes were constantly twitched towards the right. He could fix them by an effort for a short time, but the twitching soon recurred. The head was spasmodically drawn to the right side, and the right shoulder was at the same time raised towards it. There was with this a movement of rotation of the head, the chin being turned towards the point of the right shoulder, with the face looking directly over it. The spasms were at times so violent as to draw the chin behind the line of the shoulder. The sterno mastoid and trapezius muscles were thrown into strong relief during the more violent spasms. The right shoulder was always on a higher level than the left, and this gave an appearance of distortion to the body, but the spine was quite straight.

Although the sterno-cleido-mastoïd and trapezius muscles were apparently the seat of the most violent spasm, yet it was evident from the position of the head that their action was not the sole cause of the distortion. The combined action of these muscles would tend to bring the head down towards the shoulder, and to raise the shoulder itself, but at the same time to turn the chin towards the opposite side. The great pain which he suffered and the spasmodic contractions were due, probably, to the antagonistic action of several muscles—the splenius, and the inferior oblique and the greater posterior rectus dragging the face round in opposition to the actions of the trapezius and sterno-cleido-mastoïd. There was no affection of the muscles of mastication.

By a very strong effort, and aided by the pressure of his hands, he could nearly, but not quite, bring the head into its natural position; but this was in a few seconds followed by more severe spasms. Any attempt by others to restore the head to its position by external force gave rise to such violent muscular action in the neck as to make it insupportable. When the paroxysms were severe he suffered very great pain, and he was never altogether free from discomfort.

During sleep the head was sometimes, though rarely, quiet, and lay in a natural position; but generally it was twisted round, and at times the spasms came on so as to awaken him. Sometimes he was altogether prevented from sleeping by them.

There was no appearance of disease or injury about the spine; the examination would bring on more powerful action, and thus produce pain; but the same would occur if any part of the right side of the neck were handled. He complained of pain down the back; but there was no particular tenderness in any part of it. His general health was impaired by the constant pain and loss of rest.

The most careful examination failed to reveal any special point of irritation which might by reflex action give rise to these spasms. My impression was, that they had a deep origin—the spinal accessory nerve, the abducens oculi, and some of the branches of the first and second cervical nerves being principally involved in their production. Whether they were reflex actions from some deep-seated irritation, perhaps within the spinal canal, or were set up by direct irritation in the nervous centres could not be determined; the former view seemed the more probable.

It would be needless to describe all that was attempted for his relief. For many months he was subjected to treatment, local and general, but with no benefit. Counter-irritation to the neck and over the spine generally, ice, and heat to the spine, galvanism, electricity, the local application of belladonna, opium, veratrine, and such like agents; the internal use of a host of sedatives, antispasmodics, and alteratives were alike impotent. The sub-cutaneous injection of morphia certainly relieved him and procured sleep; but he was not essentially better after a prolonged trial of it. Chloroform readily affected him, and under its influence the spasms entirely gave way, but they returned with all their former violence when its effects had passed off.

Then I determined to divide the sterno-cleido-mastoïd muscle. It was not a case in which the same benefit could be expected from the operation as in ordinary wry neck. But one often sees that when a large class of muscles is affected by spasm induced by local irritation, relief is given to all by the section of one of the antagonistic muscles chiefly involved. The operation was done by making a puncture at the inner side of the sternal tendon of the muscle, about a quarter of an inch from its origin, carefully carrying a blunt-ended tenotomy knife flat along the posterior surface of the muscle, feeling the resistance of its fibres the whole way, and then, turning the sharp edge towards the muscle and, rendering the fibres as tense as possible, cutting entirely through it. No bleeding took place. The extreme tension and spasm were at once markedly relaxed, but by no means entirely overcome. After twenty-four hours an attempt was made to keep the head in a more natural position by means of a collar constructed for the purpose; but although it could be brought into position with much less difficulty than before the operation, and could be fixed in it by the collar, the spasms were yet strong enough to drag the head round towards the shoulder, and the pain from the resistance of the collar was too severe to be long sustained. This treatment was soon discontinued, as it evidently did harm.

The muscle united quickly, and the spasms recurred with as much violence as before.

The man's health was giving way under the constant pain and irritation, and it was evident that it must eventually break down altogether unless some decided relief could be obtained. The sterno-cleido-mastoïd and trapezius muscles were clearly exercising a powerful traction on the head, and I thought that if their united action could be prevented that of the antagonistic muscles, even if persistent, might be controlled or tolerated. I was encouraged in this opinion by having seen the relief which the division of the sterno-mastoid alone afforded to the patient.

The division of the external branch of the spinal accessory, and the removal of a part of the nerve seemed alone to promise the desired effect, and this operation I performed in February, 1862.

Before the operation I tried to ascertain on the dead subject the best mode of reaching the nerve as it lay embedded in the upper part of the sterno-cleido-mastoïd muscle. The following seemed the easiest, and was the one which I performed with little difficulty on the patient. An incision two inches long was made along the posterior border of the muscle, the centre of the incision corresponding to about the centre of its edge. The fascia being slit up to the same extent, the trapezial branch of the nerve was sought for as it emerges from the sterno-cleido-mastoïd to cross the posterior triangle of the neck. It would be found generally a little above the centre of the incision. When found, the nerve was traced through the fibres of the muscle—the fibres being cut through much as is done in an ordinary anatomical dissection—until the common trunk above the division into the trapezial and sterno-mastoïd branches was reached, and here a piece about a quarter of an inch in length was cut out. As the operation was, of course, done under chloroform, no effect was observable when the nerve was divided, the muscles were already thoroughly relaxed from its influence. On his recovery from the effects of the chloroform the trapezius and sterno-cleido-mastoïd were found to be completely paralysed, and although there was still an occasional and slight convulsive movement of rotation of the head, it lay on the pillow in almost a natural position. There was no tendency whatever to undue action of the corresponding muscles on the opposite side. The respiration was not in any way affected, nor did he experience any peculiar sensation. All he did feel was relief from the extreme tension of the neck. The countenance was more tranquil than it had been for months. The wound healed without any trouble.

When he got up it was found that the head maintained nearly its natural position. He did not require any special support. There was still some slight action of the rotatory muscles of the head; the sterno-cleido-mastoïd and trapezius remained perfectly flaccid, except at the back and upper part of the clavicular portion of the former muscle, which was tense and evidently acted when he attempted to bring the right ear down towards the shoulder.

He soon began to regain flesh and strength. I kept him in the hospital for three months after the operation, and he was discharged in May, 1862, having been in the hospital upwards of sixteen months. On leaving the hospital he went down to the country, where he was soon able to resume his work as a labourer and thatcher.

I heard of him from time to time, and in January, 1865, I sent for him to town in order to examine into his condition. He was looking healthy, the countenance was tranquil, the face turned directly forward with the forehead and chin in a perpendicular line. Occasionally and for a few seconds there was a trifling twitch of the head towards the right side, with a little movement in the eyes. Any sudden touch or excitement would bring this on. The right arm hung listlessly against the side. The body was a little deflected from the perpendicular, so that a line dropped from the centre of the forehead fell an inch and a half to the left of the pubic symphysis. This was owing to a uniform and very trifling arching of the spinal column, the concavity being directed towards the left; there was no indication of a double curvature. The right shoulder and right nipple were about an inch higher than the left. This gave an appearance of increased size to the right side of the chest, but the measurements were the same on the two sides. The right shoulder, however, projected more from the side than the left. Measured either from the vertebra prominens or from the centre of the sternum, there was a good inch more of length to the tip of the acromion on the right than on the left side. This was due to the right shoulder being brought more to the horizontal position, while in the left shoulder the slope was perhaps greater than natural.

The right sterno-cleido-mastoïd muscle was completely wasted, except at its upper and posterior part; here for about the breadth of half an inch, and extending from behind the mastoid process to the middle of the posterior border of the muscle it was nearly as large as on the opposite side. Towards its lower end this band of fibres, which contracted strongly on his moving his head, tapered off to a point.

The trapezius was entirely wasted, a lamina not thicker than a shilling and quite flaccid could be felt in the neck. No contraction could be discovered in any part on his moving his head or shoulders. The rhomboïd muscles could be seen in action below its dorsal part. These muscles were, I think, larger than natural. On the opposite side the trapezius was largely developed. On his raising the shoulders the right was elevated by three quarters of an inch more than the left, and, although the trapezius was so wasted, the right shoulder when raised appeared fuller than the left.

There was a little rotation of the right scapula, the inferior angle being tilted upwards and outwards, and the outer angle forwards.

The right arm and forearm were as powerfully developed as the left; the deltoïds were equal on the two sides, and no difference was observable between the two great serrati.

His respiration was natural, and nothing peculiar was observed on his making a forced inspiration.

No alteration of sensibility was to be discovered in the neck and back.

This is, I believe, the only instance of resection of the trunk of the external branch of the spinal accessory, and it is consequently interesting in a physiological as well as in a surgical point of view.

The fact of the upper and posterior part of the sterno-cleido-mastoïd muscle retaining its activity may be accounted for in two ways. First, by the existence of some twigs given off from the nerve to the muscle before its division into its two main branches. This is unusual, but is not improbably the real solution. The second explanation is, that as the nerve forms numerous connections with the cervical nerves, some of the branches may have supplied this part of the muscle. This is rendered improbable by the interesting fact that although both the sterno-cleido-mastoid and the trapezial branches of the spinal accessory are freely associated with the cervical nerves, every part of both these muscles, with the exception above noticed, was completely paralysed. The muscles acted neither by volition nor in respiration.

This latter fact tends fully to confirm the observations of Dr. J. Reid and of Valentin, that these muscles do not receive their stimulus to voluntary movement through the medium of the cervical nerves. As experiments on the lower animals have abundantly shown that the spinal accessory is a purely motor and not a reflex nerve, the purpose which is served by these free connections with the spinal nerves is not quite clear. Irritation of the nerve does not cause contraction of any muscle except the trapezius and sterno-cleido-mastoïd. May it be that theyminister to the muscular sense? Or is it not probable that by means of these communicating branches the two muscles supplied by the spinal accessory are brought into associated movement with those supplied by the second, third, and fourth, and even perhaps the fifth cervical nerves, amongst others the diaphragm and the serratus magnus? Regard being had to the generally admitted function which these muscles exercise in respiration, it is most likely that some such association must exist.

The rotation and elevation of the scapula were probably due to the action of the serratus magnus—unbalanced by the trapezius—but in some degree limited by the increased action of the rhomboïds. The rhomboïd muscle would prevent the serratus from drawing the scapula too much forwards, but would tend at the same time to elevate it; and the serratus magnus itself would, I believe, raise the outer angle of the scapula, as well as bring it forward if uncontrolled by the trapezius.

The position of the body was a natural result of that of the shoulder. As the axis of the right shoulder from the spine to the acromion was an inch longer than that on the left side, the arm would hang at the end of a longer lever. To compensate for this the body would necessarily be somewhat arched to the opposite side, as is done whenever the arm is kept extended.

In a surgical point of view, the case is of interest as one of unusual severity and involving a large class of muscles. The pathology of wry neck from muscular action is but imperfectly understood. My belief is, that the complaint is due to an irritation of the nerves in every instance in which inflammation or some disease of the muscles themselves has not preceded it. I doubt much whether, as is often alleged to be the case, it is ever caused by paralysis of the muscles of the opposite side. In the normal state of the muscles the paralysis of one class does not give rise to increased contraction of their antagonists. In facial paralysis the features are twisted; because in the actions of speaking and in the expression of the emotions the one set of muscles only is brought into play, and there is no power in the paralysed muscles to oppose them or restore the equilibrium of the features. But even here how commonly do we see the features maintaining their equilibrium unless disturbed by such emotional or volitional actions? After section of the tendons we do not find that a limb is drawn into an opposite direction.

In the present case, of course, there could be no question as to such a cause. The spasms and the twisting of the neck were clearly due to some irritation, affecting the nerves of a large class of muscles, and probably of a reflex character. The irritation was kept up by the very antagonism of the muscles brought into action. Had the seat of irritation been discoverable, and within the reach of treatment, no doubt the whole train of symptoms might have been combated.

What ground was there, then, for believing that by paralysing one set only of the spasmodically affected muscles relief would be given to the spasms in the other? Had the spasms had a central origin in the cord, I do not know that much benefit would have followed the operation. But there was the evidence that the symptoms were partially relieved so long as the sterno mastoid remained ununited after its division. And it is well known that where a large class of muscles is involved in spasmodic action, arising from reflex irritation, the section of a single muscle will entirely remove it. In a paper which was published in the thirty-third volume of the 'Medico-Chirurgical Transactions' on the section of the tendo Achillis in cases of fracture of the leg, complicated with spasms of all its muscles, this was clearly shown; and the operation has been often repeated with equal benefit. Had the cause of the irritation in the present case been of a merely temporary nature, the simple division of the sterno-cleido-mastoïd would perhaps have been sufficient to quiet the muscles generally. The return of the spasms in all their former force, when the muscle had reunited, showed that little good could be expected from any temporary arrest of muscular action; and hence I resolved to paralyse completely and permanently the two powerful muscles which were chiefly involved, and the nerves of which were within reach.

With regard to the operation itself I believe it will be found, should it ever be repeated, that the method I have indicated would be the simplest. The point at which the trapezial branch emerges from the sterno-mastodeïdo muscle varies to the extent of an inch or more; but it can readily be found by exploring the edge of the muscle, and can then be easily traced back to the common trunk. There would be far more difficulty in finding the trunk by section through the upper part of the sterno-mastodeïdo; for its situation is variable—sometimes it lies deep within the muscle, sometimes it lies close upon the inner surface. The attempt to reach the nerve by sub-cutaneous incision would, I think, be hardly advisable.

Art. IV.

On the Non-identity of the Parasites met with in Favus, Tinea Tonsurans, and Pityriasis Versicolor, including proofs derived from the occurrence of these diseases amongst the lower animals, and their transmission from them to man. By Dr. McCall Anderson, Lecturer on Practice of Medicine in Anderson's University, Physician to the Dispensary for Skin Diseases, &c. *Glasgow.*

It is curious to note the variety of opinion which prevails amongst scientific men as regards many points relating to the so-called vegetable parasitic affections of the skin. Thus some, with Wilson at their head—whose opinions must always command respect[1]—hold that there are no such diseases, the plant-like structures met with in favus, ringworm &c., not being fungous growths at all, but mere degenerations of the normal elements of the skin. Others, while admitting the presence of fungi in these diseases, hold that they are not essential, but accidental formations; and many are of opinion that they are not peculiar to them, but are met with more or less in almost all chronic skin diseases.[2] Then there are those, with Devergie for their leader,[1] who lean to the theory of spontaneous generation as applied to them; and, lastly, the camp is pretty equally divided between those who believe that several fungous growths are concerned in the production of the parasitic affections of the skin, and those who maintain that they are due to the presence of one and the same parasite.

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[1] "On the Phytopathology of the Skin and Nosophytodermata, the so-called parasitic affections of the skin."—'British and Foreign Medico-Chir. Review," Jan., 1864. See also a pamphlet in answer to this paper entitled 'The Nature of so-called Parasites of the Skin,' by W. Tilbury Fox, M.D. T. Richards, 37, Great Queen Street. 1864.

[2] See an article by Mr. Jabez Hogg, in the 'Lancet' for March 26th, 1859.

It has been urged by some, whose opinion I value, that, in the volume published by me some years ago on the parasitic affections of the skin, this last point should have been fully discussed; but it appears to me that, in a work intended as a guide to diagnosis and treatment, it would have been wrong to have entered into details on this head, except in so far as they were necessary to the practical elucidation of the subject. In the volume alluded to, however, I endeavoured to prove the correctness of Bazin's view, which was contrary to the belief of dermatologists in this country—that herpes tonsurans (ringworm of the head), herpes circinatus (ringworm of the body), and Sycosis parasitica (ringworm of the beard), are all due to the presence of one and the same parasite, the Tricophyton;[2] and all my subsequent experience has tended to confirm the opinion which I then expressed, an opinion which, it is gratifying to observe, has been pretty generally accepted by the profession. There are not a few, however, who go farther than this, who hold that there is only one parasite productive of *all* the vegetable parasitic affections of the skin, amongst whom may be mentioned the names of Hebra, Tilbury Fox,[3] Lowe, and Jabez Hogg,[4] to whose writings I must refer the reader for the arguments in favour of such an opinion, as the following pages are devoted almost exclusively to the arguments in favour of the opposite view.

But, before proceeding further, it may be well to state that, as there is a difference of opinion amongst those dermatologists who admit a group of parasitic affections of the skin, as to whether Alopecia areata (Porrigo decalvans) is a parasitic disease or not, it is advisable to leave that affection out of consideration in the present discussion in order to avoid confusion. So that the task which I propose to myself now is to lay before my readers the arguments in favour of the view that the *Trico-*

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[1] 'Traité pratique des Maladies de la Peau' par Alph. Devergie, ed. ii, pp. 51 and 501.

[2] 'The Parasitic Affections of the Skin,' by T. McCall Anderson, M.D. p. 46. London: Churchill, 1861.

[3] 'Skin Diseases of Parasitic Origin,' by W. Tilbury Fox, M.D. p. 99 et seq. London: Robert Hardwicke, 192, Piccadilly.

[4] "Further Observations on the Vegetable Parasites, particularly those infesting the human skin." By Jabez Hogg, F.L.S., M.R.C.S., &c.—'Quarterly Journal of Microscopical Science,' January, 1866, p. 10.

phyton, the parasite met with in the three varieties of ringworm (viz., herpes tonsurans, herpes circinatus, Sycosis parasitica), the *Achorion Schönleini*, the parasite of favus, and the *Microsporon furfur*, the parasite of pityriasis versicolor, are not identical, but distinct fungous growths.

First of all, let us view the proofs of non-identity, as these are displayed in the *results of inoculation*.

(1.) *Results of inoculation with the Achorion Schönleini* (the parasite of Favus). This parasite has been repeatedly inoculated with success, and, amongst others, by Hebra, Rémak, Vogel, Bazin, Gruby, Köbner, and Deffis. Bennett thus describes a case in point—

> "In the summer of 1845 one of the gentlemen in attendance at the Royal Dispensary volunteered to permit his arm to be inoculated. A boy, called John B—, æt. eight, labouring under the disease (favus) was at the time the subject of lecture, and a portion of the crust, taken directly from this boy's head, was rubbed upon Mr. M—'s arm, so as to produce erythematous redness, and to raise the epidermis. Portions of the crust were then fastened on the part by strips of adhesive plaster. The results were regularly examined at the meetings of the class every Tuesday and Friday. The friction produced considerable soreness, and, in a few places, superficial suppuration. Three weeks, however, elapsed, and there was no appearance of favus. At this time there still remained on the arm a superficial open sore, about the size of a pea, and Mr. M— suggested that a portion of the crust should be fastened directly on the sore. This was done, and the whole covered by a circular piece of adhesive plaster, about the size of a crown-piece. In a few days the skin surrounding the inoculated part appeared red, indurated, and covered with epidermic scales. In ten days there were first perceived upon it minute bright yellow-coloured spots, which, on examination with a lens, were at once recognised to be spots of favus. On examination with the microscope, they were found to be composed of a minute granular matter, in which a few of the cryptogamic jointed tubes could be perceived. In three days more the yellow spots assumed a distinct cupped shape, perforated by a hair; and in addition to tubes, numerous sporules could be deected."[1]

Of three cases inoculated by Deffis, the epidermic variety of favus—the crusts exhibiting the Achorion microscopically—was produced twice, and a typical favus cup once, and the average period of incubation was ascertained to be about forty days. The true favus cups are only formed when, by inoculation, some of the fungus can be brought into contact with a hair-follicle; hence the epidermic variety is more frequently produced. Köbner inoculated himself on the forearm with the parasite of favus, and there resulted well-marked favus cups,[1] which he exhibited at the Medical Society at Breslau. Gruby also tried the effects of inoculation. He deposited some of the fungus on the bark of an oak in full vegetation, and there developed itself a favus cup identical with that which grows on the head of infants, and which was exhibited at the French Institute.[2]

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[1] 'Clinical Lectures on the Principles and Practice of Medicine,' by J. Hughes Bennett, M.D., ed. ii, p. 799. Edinburgh: Adam and Charles Black.

(2.) *Results of inoculation with the Tricophyton* (the parasite of tinea tonsurans, or ringworm.)—The experiments with this parasite have been on a much less extensive scale than those with the achorion, but, as far as they go, they lead to the same conclusion. Thus M. Deffis, encouraged by the success of his inoculations with favus matter, essayed some inoculations with the tricophyton in 1856, in which he was completely successful, characteristic patches of ringworm being produced; and similar inoculations were made with the tricophyton by Köbner on his own and on Dr. Strube's forearm, and also upon rabbits, which resulted likewise in the development of ringworm.[3]

(3.) *Results of inoculation with the Microsporon furfur* (the parasite of pityriasis versicolor.)—The inoculation of the Microsporon furfur has not, as far as I am aware, been attempted, or at all events the results have not been communicated by any one, except by Dr. Heinrich Köbner, who inoculated himself with it upon the skin covering the sternum, and produced an eruption of pityriasis versicolor.[4]

Now of all the inoculations which have been made upon man, animals, or plants, with the achorion, the tricophyton, and the *Microsporon furfur*, many, of course owing to defective inoculation, unsuitableness of soil, or the like, have proved abortive: but I think I am equally correct in stating that amongst the many cases of successful inoculation, not a single one has resulted in the production of any other parasitic disease than that from which the parasite was taken. In other words, when the inoculations were successful the achorion always gave rise to favus, the tricophyton to Tinea tonsurans, and the *Microsporon furfur* to pityriasis versicolor.

So much, then, for the results of inoculation.

In the second place, let us glance at the *clinical proofs* of the non-identity of these parasites.

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[1] 'Klinische und Experimentelle Mittheilungen aus der Dermatologie und Syphilidologie,' von Dr. Heinrich Köbner Arzt in Breslau. p. 21. Erlangen, 1864.

[2] 'Traité pratique des Maladies de la Peau,' par Alph Devergie, ed. ii, p. 526.

[3] 'Klinische und Experimentelle Mittheilungen aus der Dermatologie und Syphilidologie,' von Dr. Heinrich Köbner Arzt in Breslau. p. 23. Erlangen, 1864.

[4] Ibid., p. 24.

There are very few dermatologists of note who now deny the contagious nature of favus, tinea tonsurans, and pityriasis versicolor. Amongst the 1300 cases of parasitic affections of the skin treated at the Dispensary for Skin Diseases, Glasgow, during the last four years, there were numerous examples of this, but there was not a single instance of one of those diseases giving rise, by contagion, to one of the others. And this is just what one would have expected, seeing that artificial inoculations point so conclusively the same way. And here it must be mentioned, that those who are not well versed in the diagnosis of skin diseases are apt to fall into the error of confounding the appearances of the first stage of favus with fully developed ringworm, and thus to arrive at the opinion that these two diseases are present on the skin at the same time. That there are instances of the coincidence of ringworm and favus on the same person at one time—a delineation of which is published by Hebra—no one can deny, but it is equally certain that they are very rare, for I have never met with a single case of the kind; so that they no more constitute proofs of the identity of these diseases than do instances of the coexistence of psoriasis and ringworm—a case of which I met with the other day—of the identity of these two affections. Then, if we study the appearances of fully developed favus, tinea tonsurans, and pityriasis versicolor, it would be difficult to name any three skin diseases which are more dissimilar; and this I may say with the greatest confidence, that I have never seen a transition of one of these diseases into one of the others. It is but fair, however, to state that my experience differs in this respect from that of Dr. Tilbury Fox, who makes the following remarks:

> "Tinea favosa (favus) can be produced from bad cases of tinea tonsurans, on a minor scale, by keeping up such an amount of irritation as, being less than sufficient to destroy the fungus, shall lead to the effusion of blastematous fluid (be it pustular, vesicular, or other), in which the plant will vegetate rapidly for a while, producing a crust depressed in its central part, and completely riddled by hairs in various stages of disease; the crust itself being composed of the normal elements of the part, effused fluid, and parasitic growth."[1]

As I have just said, this state of matters is totally at variance with my own experience, and I cannot help suspecting that some error has crept into the inquiry.

In the third place, let us view for a moment the proofs derived from a *microscopic examination*, which I hold, however, to be of very secondary importance, and which cannot have nearly the same weight as several of those previously advanced, for in structures so minute it is difficult, even with all the light which is shed upon them by the most perfect instruments, to appreciate with precision the differences which may exist between them. And yet, as far as my experience goes, the differences between the microscopical appearances of the *Achorion*, the *Tricophyton*, and the *Microsporon furfur*, are very considerable. Thus, to take an instance derived from the spores: those of the Achorion are, on an average, about the 3000th of an inch in diameter, and many of them are oval; those of the Tricophyton, on the other hand, are much smaller, being, on an average, about the 7000th of an inch in diameter, while the spores of the *Microsporon furfur*, although nearly as large as those of the Achorion, are more uniformly rounded, and have a remarkable and characteristic tendency to run together, so as to form clusters, like bunches of grapes. Other differences in the microscopical appearances I might mention, which must be familiar to those who have carefully studied the subject of parasitic diseases of the skin with the microscope; but it is unnecessary to enlarge further on this subject, holding, as I do, that the proofs derived from a microscopical examination are of secondary value in the determination of the point at issue, and I conclude with the observation, that if carefully prepared microscopical specimens of the *Achorion*, the *Tricophyton*, and the *Microsporon furfur*, and of these only, were handed to me, and I were allowed to use my own microscope, I think I could generally arrive at a correct diagnosis of the disease from the microscopical appearances alone.

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[1] 'Lancet,' September 10th, 1859.

Curiously enough, the morning after the above was written, my friend, Dr. Irvine, handed to me a paper containing some epithelial scales and fine hairs, with the request that I should examine them with the microscope, and give him my opinion of the nature of the skin disease from which they were taken. This I did, and pronounced it to be a case of pityriasis versicolor, an opinion which proved correct. This fact is cited, not to show that I am possessed of any extra skill in the use of the microscope, but merely in verification of the above statement.

Lastly, we come to the proofs derived from a branch of inquiry in which I have for some time been deeply interested, namely, *the occurrence of vegetable parasitic skin diseases amongst the lower animals, and their transmission to the human subject*. And first of all, as regards favus, I may be allowed to transcribe the following case from a previous communication, a case which was first published by Bazin.[1]

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[1] 'Leçons Théoriques et Cliniques sur les Affections Cutanées Parasitaires,' par le Docteur Bazin (1858). p. 119.

> "In the course of the year 1854 several members of a family, amongst whom was a young physician, remarked that several mice, caught in a trap, were affected with a peculiar disease. Upon the head and front legs there were crusts of a sombre yellow tint, of a regularly circular form, and more or less elevated above the level of the neighbouring healthy parts. A manifest depression was likewise detected in the centre of each crust, just as one observes in porrigo favosa, and the parts where these had fallen off were ulcerated, and the skin appeared to be destroyed throughout its whole thickness. These mice were given to a cat, which exhibited some time afterwards, above the eye, a crust similar to those on the mice. Later still, two young children of the family, who played with the cat, were successively affected with the same disease, yellow crusts making their appearance on several parts of the body, on the shoulder, face, and thigh. The physician who was summoned pronounced them to be cases of porrigo favosa."

Some of the fragments were sent to Bazin, who detected the parasite with its characters well marked.

The following cases, which came under my own observation, are of much interest.

A patient of my late colleague's, who lived in lodgings in a newly-built house in the West end of Glasgow, showed him his dog, upon whose fore-paw a peculiar disease existed. Dr. Buchanan examined the patch, and found that it corresponded in every particular with a patch of favus—an opinion which was amply corroborated by a microscopic examination of a portion of the crusts. This dog was in the habit of killing mice which abounded in the house, some of which were accordingly caught and examined by Dr. B. and myself. We had no hesitation in pronouncing the disease to be favus, and a microscopic examination showed distinctly the presence of the *Achorion Schönleini*.

This disease in mice has a special tendency to attack the ears, and from thence it spreads to the head and throat, and to other parts. It produces much greater destruction than in the human subject, as it not only destroys the hair but tends to eat into the deeper structures, and by slow degrees leads to exhaustion and death. One of the mice above referred to was stuffed, and is preserved at the Dispensary for Skin Diseases, Glasgow, where those who are interested may have an opportunity of studying the appearances and of verifying the conclusions to which we arrived with regard to the nature of the disease. The complaint in mice had at this time attracted the attention of non-professional persons in Glasgow, as was evidenced by a correspondence in the columns of the 'Glasgow Herald,' the writers having all seen in their houses mice so affected, and having been much alarmed lest they might be the means of poisoning the food or water, or of transmitting the disease to members of their family. No instance, however, was cited in which this had occurred. A still more interesting case than that above referred to occurred shortly afterwards in my own practice. A poor woman came to the Dispensary for Skin Diseases on the 1st February, 1864, accompanied by one of her children. They were both affected with favus of the non-hairy parts of the body. On each there were scattered here and there characteristic little round patches of eruption, on some of which numerous minute favus cups were detected, exhibiting the *Achorion Schönleini* microscopically. Two other children of this woman, as also their father, were similarly affected. Mice abounded in the house some time previous to this, and a cat was accordingly procured which killed all of them. I had therefore no opportunity of examining them, but the cat was brought to me, and on the tops of its fore-paws I detected numerous undoubted favus cups.

The next case, which is equally interesting, came under my notice a few days after the last. On the 22nd February, 1864, I was asked by Mr. Thomas Bryce, surgeon, to visit along with him a family which he was attending. A number of mice had been caught in the house three months previous to this date, which had been much handled by the children. Five weeks afterwards an eruption was noticed on one of the little girls, which spread to one of the sisters, her mother, the baby, and a little girl who worked in the establishment. On examining the eruption, which was confined to the non-hairy parts, it was found to correspond exactly with the appearances in the previous case. On some of the patches distinct favus cups were seen which exhibited the achorion microscopically, and on those which were devoid of them the eruption corresponded to the variety described in my volume on the parasitic affections of the skin as 'Favus of the Epidermis,' and the scales were loaded with the spores and tubes of the parasite. There were no mice in the traps at the time, but shortly after my visit Dr. Bryce kindly sent me five, on the back of one of which near the tail a characteristic favus cup was seen, while the side and lateral aspects of the head and ears of another were eaten away by the disease. The crusts were examined with the microscope, and the achorion was detected in great abundance. Dr. Bryce informed me that the mice sent to me exhibited the same appearance as those with which the children had playing.

But favus is not limited to cats and mice, for we read that Müller observed it in a cochin-china fowl and in several chickens which had contracted it from the fowl; that Gerlach observed its transmission from fowls to the human subject,[1] and that Köbner succeeded in producing favus in rabbits by inoculating them with the achorion taken from the human subject; and there can be little doubt that as the question becomes more thoroughly ventilated, this disease will be found to be much more generally diffused amongst the lower animals than many suppose.

Now, in all these cases favus transmitted favus, and I have never read of, still less have I ever observed, any case in which either tinea tonsurans or pityriasis versicolor was the result.

Let us now glance for a moment at the occurrence of Tinea tonsurans (ringworm) amongst the lower animals, in order to see if it gives us any information upon the point at issue.

In a paper on "Parasitic Skin Diseases in the Ox," by Gerlach, Professor at the Royal Veterinary School of Berlin, the author gives an account of ringworm in oxen. Having remarked that oxen which were put into the same stable with affected ones contracted the same disease, he determined to perform some experiments with the view of ascertaining whether it really was communicable to other animals. By successive inoculations he succeeded in the production of ringworm in oxen, in calves, and in horses, while his experiments in the case of pigs and sheep yielded a negative result. He likewise inoculated his own arm and those of some of the pupils with some of the parasitic matter from oxen, and in each case there resulted well-marked herpes circinatus (ringworm of the body).

Bärensprung's experience coincides with that of Gerlach. He rubbed on his forearm some scales containing an abundance of the spores and mycelium of the Trycophyton taken from a case of ringworm in one of the lower animals. No effect was produced for the first few days, but after a longer interval his attention was attracted to the part by the supervention of itching, when he discovered a well-marked patch of herpes circinatus (ringworm of the body).[2] It is unnecessary to multiply cases of this kind, so that I may conclude with a case extracted from the volume published by me on the 'Parasitic Affections of the Skin,' and quoted from Bazin—

> "A dragoon came to the dispensary of the St. Louis Hospital, affected with herpes circinatus of the front of the right forearm; the skin of one of the patches was denuded of hair. He stated that five or six of his comrades had contracted this affection, as well as

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[1] 'Klinische und Experimentelle Mittheilungen aus der Dermatologie und Syphilidologie,' von Dr. Heinrich Köbner. Erlangen. 1864. pp. 26 and 27.

[2] Quoted by Aitken, from 'Brit. and For. Med.-Chir. Review,' July, 1857, p. 263.

himself, from grooming diseased horses. We went to the barracks, where, sure enough, we saw three horses which exhibited round patches, absolutely identical with those of herpes tonsurans (ringworm of the head) on the withers, shoulders, back, and belly. The hairs in the centre of each patch were broken off close to the skin, and there was, as in herpes tonsurans, a whitish, squamous, and even crust-like production which was traversed by the hairs. The presence of spores was detected with the microscope. The dragoon, who conducted us to see the horses, showed us also his young daughter, eight or ten years of age, the side of whose nose exhibited a patch of herpes circinatus."

We see, then, that as in the previous cases favus invariably transmitted favus, so in this tinea tonsurans invariably gave rise to tinea tonsurans.

I believe I am correct in stating that pityriasis versicolor has not been observed in the lower animals.

The following is a summary of the proofs adduced in favour of the non-identity of the *Achorion Schönleini,* the Trycophyton, and the *Microsporon furfur,* the parasites met with in favus, tinea tonsurans, and pityriasis versicolor respectively.

(1.) In all cases of successful inoculation with the *Achorion, Trycophyton* and *Microsporon furfur,* the same parasitic disease has been produced as that from which the parasite was taken.

(2.) Of the innumerable cases *occurring in the human subject* illustrative of the contagious nature of favus, tinea tonsurans, and pityriasis versicolor which have been recorded, there is no authentic case in which one of these diseases gave rise to one of the others.

(3.) The difference in the appearance of favus, tinea tonsurans, and pityriasis versicolor, when fully developed, is so very striking as to lead to the belief that they are produced by separate parasites.

(4.) There is no authentic instance on record of the transition of one of these diseases into one of the others.

(5.) The difference in the appearance of the achorion, tricophyton and microsporon furfur is sufficiently striking to enable the observer in many cases to form a correct diagnosis from the microscopic examination alone.

(6.) Of the numerous instances on record of the transmission of favus and tinea tonsurans *from the lower animals* by contagion or inoculation, favus has always given rise to favus and tinea tonsurans to tinea tonsurans.

Before taking leave of our readers it may be well to refer to the opinion of Dr. John Lowe and others, that not only are the parasites in question identical, but also that they are one and the same with the *Aspergillus glaucus.* In confirmation of this view, Dr. Lowe states, amongst other observations, that he placed in a bottle, exposed to a moderately cool atmosphere, a solution of brown sugar and some favus matter. In rather more than a month the aspergillus glaucus was detected in the solution, having been apparently developed from the favus matter. Dr. Lowe seems to have repeated the experiment several times with a like result. It must be remembered, however, that there are many sources of fallacy in experiments of this kind, and I am entirely at one with Dr. Lowe in the following remarks—

> "In an investigation of this nature, where the objects to be examined are so minute, a considerable degree of difficulty is naturally experienced in affording satisfactory proof of the accuracy of the remarks concerning their development. For instance, in watching the germination of any given fungus, it may often be difficult to prove that no other plant of the same tribe is present to complicate the result; and this in consequence of the myriads of spores of various species which are constantly floating about in the atmosphere, ready to become located, and grow upon any suitable pabulum."[1]

Moreover, similar experiments were conducted by Rémak, who did not arrive at any definite conclusion; while Köbner subjected the point to a more practical and satisfactory test by inoculating himself, Strube and others repeatedly with the penicillium glaucum, using the same precautions, as in the experiments alluded to in a previous part of this paper, but without the slightest result. Now, if the penicillium glaucum were identical with the parasites of favus, ringworm, and pityriasis versicolor, one would naturally have expected that he would have been as successful with it as he was in his inoculations with the *Achorion Schönleini*, the *Trycophyton*, and the *Microsporon furfur*. So that, while no one can withhold from Dr. Lowe the credit which is due to him for the interesting experiments which he has carried out, and for the scientific manner in which he has conducted them, I think it must be conceded that further proof is required before we can admit that the parasites productive of favus, tinea tonsurans, and pityriasis versicolor are identical with the *Aspergillus glaucus*.

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[1] 'Transactions of the Botanical Society,' vol. v, part iii, p. 193.

ART. V.

On some of the Hospitals of Northern Germany and the adjacent Countries, with Remarks on Hospital Construction, and on the Plans to be adopted in the new St. Thomas's Hospital.

FOR some years, when travelling on the Continent, I have been in the practice of visiting the hospitals and other public medical institutions which have come in my way; and during the last autumn, when in northern Germany and Denmark, I followed my usual plan. I propose, in this paper, to give an account of the hospitals then seen, believing that, at the present time, when hospital construction is attracting much attention, my remarks, brief though they must necessarily be, will not be without interest.

In Hamburgh I visited two hospitals, the General Hospital, Allgemeines Krankenhaus, and the Hospital for the Jewish Community. Both these are situated in the suburbs of the city, and are pleasantly placed near the Grosse Alster. The central portion and part of the wings of the General Hospital were erected in 1823, and an extension of the latter was completed in 1848; and the hospital, as it now exists, is one of the largest in Europe, being capable of accommodating about 2000 patients. The building is constructed on the plan which is generally adopted in northern Germany. It consists of a large front, in which are placed the administrative offices of the establishment on the lower floors, and which is occupied by patients above. At the ends of this block of building wings project at right angles to it behind, and thus a large garden is partially enclosed, in which are placed the kitchen, wash-house, post-mortem room, &c.

The wards occupy the outside of the building, and communicate throughout by means of a wide corridor on the other side. They have windows at one end and the door into the corridor at the other, and on each side doors into the adjacent wards. The ordinary wards contain twelve beds, arranged regularly on the sides, with two cabinets at the end next the corridor, one for the attendant, the other containing a water closet. They therefore contain thirteen beds, and are stated by Dr. Tüngel, in a report published by M. Husson, to be forty and a half feet long, twenty-four feet wide, and thirteen feet high, the allowance of cubic space to each bed being thus 962 feet, or, supposing the dimensions to be given in Hamburgh feet, 825·2

English feet. The windows at the end of the wards open so as to admit of ventilation; but in addition to these, apertures are provided for the admission of the external air under the floors, by channels which bring it in contact with the stoves, so as to ensure its being warmed before escaping into the ward. The wards are warmed by the usual porcelain stoves used in German houses, and the heat of the smoke vent is made the means of creating an upshot current for the removal of the vitiated air. In addition to the larger wards, there are smaller ones for the reception of noisy patients or such as otherwise require seclusion, and for patients who pay for their treatment. In the wings complete sets of baths are provided. The wards are placed on two floors—the ground and first floors; but, in consequence of the want of accommodation, the basement was in some parts occupied by beds at the time of my visit.

From information furnished to M. Husson by Dr. Tüngel, the principal resident medical officer, to whom also I am indebted for much polite attention, it appears that the number of patients in the hospitals on the 31st of December, 1860, was 1761, and the number admitted during that year amounted to 5982, the cases being, as the name indicates, of all classes. The cost of the establishment is partly defrayed by the patients themselves, and partly by different charitable institutions by which they are sent to the hospital, the deficiency being supplied from the funds of the city.

The hospital for the Jewish community is situated not far from the General Hospital. It consists of a centre without wings, but is otherwise similarly constructed to the General Hospital, the wards being situated in the front of the building and connected together by a corridor at the back. The rooms on each side of the central hall are devoted to the residence of the superintendent and to the various offices, and the patients are placed in wards above and at the ends of the corridor. These apartments contain eight, five, four, and two beds each, the total accommodation amounting to about 100 beds, and the space allotted to each patient being, according to the published report of the physician, Dr. Heilbut, about 1000 Hamburgh cubic feet, or 940 English. The hospital has a south aspect, and is a model of cleanliness and neatness, the only objection to it being that, as in the General Hospital, the cabinets for the attendants and the water closets are placed in the wards at the entrance from the corridors. The hospital was built in 1840, at the sole expense of Solomon Heine, a banker in Hamburgh, to replace an older establishment, no longer fitted for the purpose; and it is called, in memory of his wife, the "Betty Heine Stift." It has since been endowed by his son, so that the establishment is now entirely independent of any external support. It is, however, open to the reception of persons who defray the whole or a part of the expense of their treatment; and though designed for the Jewish community, it is not limited to them, other persons being admissible if there be room in the establishment and they be willing to conform to the regulations.

In Copenhagen there are three large general hospitals. Two of these are old, having been built in the last century. They are both situated in the new part of the city, near the Amalienborg, and are respectively adapted for the reception of about 400 patients; one of them, the Fredericks Hospital, I visited. In this institution the patients are placed in small wards on the ground and first floors, which have windows at the sides, and communicate with each other and with the courtyards round which they are built directly or by passages and stairs. The wards are low and the space deficient, and altogether there is little to approve in their construction. Such is not, however, the case with the third hospital to which I have referred—the Kommune hospitalet. This has only recently been erected, having been opened for the reception of patients in September, 1863. It is situated in the suburbs of the city, outside the fortifications, not far from the north port and railway station, and is in every respect an admirably constructed and well-managed establishment, and may safely be referred to as one of the best hospitals in Europe. It is built of brick in alternate layers of a white and pale-brown colour, and, though perfectly plain and simple, is in appearance an effective building. It consists of two long blocks three stories in height, placed parallel to each other, and having, in front, a south-west aspect. Between these blocks two others run across, thus forming three courts laid out as gardens. The central one is entirely surrounded by the hospital buildings, while the two others are bounded by them only on three sides, and are open in the fourth. These gardens are large, and there is every appearance, from the condition of the trees and shrubs, that they get a full amount of fresh air and sunlight.

The wards are constructed as in the hospital at Hamburgh, the front being provided with an ample amount of window space, and they communicate with the corridor by the door at the other end; but, in addition, there are windows with swing sashes opening into the corridor, so as to afford free ventilation. The larger wards contain ten beds, and are about thirty Danish feet long, twenty-four wide, and fourteen high, thus giving an entire capacity of about 1008 Danish feet to each bed, or 1099·4 English cubic feet; but, in addition to these wards there are about 100 others of smaller size, for one, two, three, and four patients each, of which the relative capacity is greater; so that Dr. Thellussen, to whom I am indebted for much information and kind attention, estimates the average space per bed, in the hospital at large, at 1200 cubic feet, or 1235 English. The width of the corridors is ten feet. The floors throughout the building are constructed of stained and polished deal; the staircases are of wood on iron supports, and are large and handsome; and there is a lift for the conveyance of patients to the upper wards, but this is not much made use of. The wards are provided with stoves, and the air which is admitted from without is warmed by passing through tubes in connection with the stoves before being distributed; whilst the vitiated air is drawn out through wooden pipes or canals, in which a draught is created by an "aspirating machine," a fan moved by steam power. These passages open at the top of the building. The corridors, chapel, operating theatre, counting-house, &c., &c., are warmed by hot water. The nurses' rooms are placed between each pair of wards, with a separate window and door into the corridor, and also have windows into the wards. On the outer side of the corridors, or opposite the wards, there are small projections into the garden, in which baths, lavatories, and closets are placed, in the proportion of one set for each five wards. In addition, however, to the means of bathing here provided for patients who cannot bear removal to any great distance from their wards, there are two very complete bathing houses, which are situated in the garden at the back of the second block of buildings, and near the furnaces for the steam engine, and these communicate directly with the hospital.

Behind the main hospital, and entirely separated from it by a wide garden, are two additional buildings, one appropriated to the reception of smallpox cases, the other of acute cases of lunacy. The former of these appendages contains forty beds, the latter twenty cells; but lunatics are only treated in this establishment till they are capable of being conveyed to an asylum in the country.

Between the two last-named buildings, and quite apart from both of them and from the hospital, are situated the dead-house and post-mortem rooms. This building is of only one story, and consists of four chambers, one for the reception of the bodies when first removed from the wards; another, which opens from it for the ordinary dissections; a third which communicates with the last for special examinations; and a fourth, opening from the first, for the bodies to be placed in before interment, and where they can be visited by the friends without having communication with any other part of the establishment. This arrangement ought, I think, to be copied in our hospitals. At the time of my visit two bodies were lying on biers waiting for removal.

I was much pleased with the construction of the operating theatre. It is situated on the ground floor, and is lighted by a large window in front. Before this the operating table is placed, and at each side, in rapidly ascending rows, are the seats for the students. At the back there is direct access to the entrance hall and wards of the hospital. The arrangement seemed to me to afford great facilities for operations being seen by a large number of spectators.

At each end of the front portion of the main building are small houses, one for the superintending surgeon, the other for the chaplain. This arrangement also seemed very good, affording as it does ready access to and from the wards, with entire separation of the officers' families from the inmates of the hospital.

The whole establishment affords accommodation for 800 patients, but has never received more than 760, and then only during the prevalence of special epidemics. It was constructed at a cost of £170,000 sterling, on a loan from the city of Copenhagen, and the expense of the maintenance of the establishment is defrayed partly by the patients who pay for their treatment, and partly by an income-tax on the residents of the city. The former source supplies about one third of the whole sum expended.

Dr. Thellussen informs me that they attend at the hospital, as out-patients, any persons who apply for advice; but there are also in Copenhagen medical men who visit such patients as require it at their homes. For this purpose the city is divided into seventeen districts, to each of which a medical officer is attached, who furnishes both medical attendance and medicine to the poor, at the expense of the municipality. There is also a sick ward in the workhouse; but in this only slight cases are treated, the more severe being removed to the Communal or other hospitals.

I may further mention that when in Berlin I met with a gentleman who stated that he had been attached to the latter hospital during the Schleswig-Holstein war, having had charge of the wounded Prussian prisoners; and he bore very satisfactory evidence to the efficiency of the ventilation of the hospital, stating that the cases did much better there than in the other Copenhagen hospitals.

In Berlin I visited four hospitals, the Charité, Bethany, Catholic, and Jews' Hospitals. The Charité is a large establishment, embracing departments for the treatment of cases of all descriptions of disease, including a separate lunatic asylum and lying-in hospital, &c. Altogether it is capable of accommodating about 1400 patients. It is constructed on the plan of the hospital at Hamburg, with a long front and wings at right angles behind, having large gardens at the back. The wards are also similarly arranged, and communicate throughout by long and wide corridors. The number of beds in the different wards varies from eight or ten to sixteen or twenty. The windows at the ends of the wards are made to open for ventilation, and the wards are warmed by stoves and the corridors by hot water. The space allotted to each bed, according to the information given by M. Husson, varies considerably, being from 1400 to 1600 cubic feet. Behind the hospital are situated the microscopical and chemical examination-rooms and the pathological museum, under the care of M. Virchow.

The Bethany is only of recent construction, having been founded by the late King of Prussia in 1847. It is a small hospital, capable of receiving about 350 patients, though, from its construction and the space allowed, the building gives the impression of affording a much greater amount of accommodation. The plan is similar to that generally followed in Germany, consisting of a solid building, with wards in front and a corridor behind. The windows are made to open for the admission of air, but in addition there are apertures which pass from without to the stoves in the centre of the wards, by which the air in cold weather may be warmed before admission. There is also a system of tubes in connection with the smoke-vent, by means of which the vitiated air is removed. The wards are only of small size, the largest being adapted for the reception of ten to fourteen beds, while the smaller contain only three or four, and there are closets provided in the wards, and a cabinet for the attendants. The attendants do not, however, remain permanently in the wards, but are only there during their time of duty, and live and take their meals in a different part of the establishment. The corridors are warmed by steam. This establishment is under the charge of Protestant deaconesses, a body of females, either unmarried or widows without children, who must be between the ages of eighteen and forty, and remain attached to the establishment for a period of five years, though they are at liberty to leave after the expiration of their term, on giving satisfactory reasons for wishing to do so. They are under the charge of a lady superior and a chaplain, and are provided with medical attendance and all other requirements at the expense of the society. The Bethany is certainly one of the best hospitals which I have seen; it is scrupulously clean, and the space allowed to the patients is very ample, being, according to M. Husson, in the smaller wards from 1700 cubic feet to upwards of 2000, and in the larger about 1100.

The Catholic Hospital is a small building, affording accommodation for from 240 to 250 patients. It is under the charge of Sisters of Charity of the order of San Carlo Borromeo, of which the parent establishment is at Nancy, and which has also a hospital at Treves. The institution is partly supported by voluntary contributions and partly by persons subscribing for the privilege of presenting patients to one or more beds. There are also a certain number admitted who pay the expenses of their maintenance and treatment. The applicants are not restricted as to their religious views, and a large proportion—I was told about three fourths—are Protestants, and no interference with their religious sentiments is ever practised. The plan of the hospital is very similar to that in the other establishments; but in the department allotted to the private patients the corridor occupies the centre with the chambers on each side, a plan of construction which is the most unfavorable for efficient ventilation. In other respects, however, the hospital is well constructed, and it is evidently very well managed. Everything, as far as possible, is provided on the spot, there being even cows kept for the supply of milk.

The Jews' Hospital is of very recent construction, and is a beautifully finished and well-constructed hospital, on a similar plan to the hospital of the Jewish community at Hamburg; but having the great advantage over that establishment that the apartments for the attendants, and the closets, baths, and lavatories, are placed between two adjacent wards, so as to have ready access from both, instead of occupying a part of the ward itself. The wards vary in size, some being capable of receiving eight or ten beds, others only one or two, and the total accommodation is for about 100 patients. The superficial space is very ample; but the wards are somewhat low, so that the cubic space per bed is not large, according to M. Husson only about 850 feet. The corridor, which runs the whole length of the building, is nine feet wide, and there are three good stone staircases. The wards are warmed by stoves, and the corridors by hot water or steam. The establishment receives patients who pay for themselves, and others whose maintenance is provided for by the Jewish community. A system of outpatients or dispensary relief is also provided for members of the body who do not enter the hospital, at the general expense.

At Frankfort-on-the-Maine I visited also the Jews' Hospital and the General Hospital—the Heiligeist-spital. The former is only a small establishment, which, I was told, is maintained at the cost of the Rothschild family. The patients occupy apartments containing one, two, or three beds, and the whole rather partakes of the character of a private establishment than of a public hospital.

The General Hospital is of comparatively recent date, though the foundation is old. The present building was commenced in 1833, and was opened for the reception of patients in 1839. The plan of construction is that of a solid building, with a ward in front and a corridor behind; but the buildings extend round the four sides of a courtyard, and as this is not of large size—117 feet by 70 (109·8 E. by 65·7 E.)—and the buildings on three sides are three stories high, there can be very little exposure to sunlight and fresh air. Indeed, the establishment had a much less cheerful air than any other which I visited during my tour. The wards are adapted for twelve beds, of which one is occupied by the attendant; and the space provided is forty feet in length (37·56 E.), thirty in width (28·17 E.), and fifteen and sixteen in height (14 and 15 E.), thus giving a cubic space of 1500 to 1600 feet (1229 to 1317 E.). The wards are placed on the outside of the building, and the corridors on the sides of the court-yard, so that the former have a supply of fresh air directly from without. They are warmed by stoves. I was informed that a system of ventilation had originally been introduced, but, not being found to answer, was entirely abandoned, the ventilation provided by the windows and doors being now entirely relied upon. Between each pair of wards there is a passage about eight feet wide, with a window at one end and opening by a door into the corridor at the other, which I was informed was intended for assisting the ventilation of the corridor. In connection with this passage there are cabinets for patients who cannot leave their wards. In addition to the large wards referred to, of which there are eighteen, there are several smaller ones for from one to seven beds, for pay patients or those otherwise requiring isolation and quiet. The total accommodation of the hospital is for 270 patients, though only from 150 to 170 are ordinarily resident. The establishment differs from those in the other towns which I have mentioned in being very much limited to the reception of acute contagious diseases in adults—cases of smallpox, venereal affection—epilepsy and some other forms of nervous disease, and dysentery, and puerperal females being excluded, there being in the city special hospitals for several of these affections, and also for sick children. According to M. Husson, to whose work I am indebted for much of this information, there is also a system of out-door medical relief in connection with the hospital.

At Brussels there are two large hospitals, both of which I visited. The Hospital Saint Jean, in the Boulevard du Jardin Botanique, near the Northern Railway Station, is an old foundation; but the erection of the present building was commenced in 1838, and completed, so as to admit of the reception of patients, in 1843. The plan upon which it is constructed is very similar to that of the well-known Lariboisière Hospital in Paris; indeed, the latter, which was commenced in 1846, and completed in 1854, is to a great extent a copy of the Brussels hospital. The buildings of the hospital fronting the boulevard and surrounding the courtyard behind, are devoted to the administrative offices and the residences of the hospital staff. On the opposite side of the court-yard, and fronting the entrance from without, is the chapel, a beautiful and simple building, and on each side of the chapel are the corridors leading respectively to the men's and women's wards on the ground floor, and the stairs to the corresponding wards on the first floor. The corridors form the sides of a large and wide garden, of which the one end is closed by the chapel and a passage in the rear, the other being open to a wide garden behind. The wards are placed at right angles to the corridors, and form separate blocks of buildings, surrounded at the sides and ends by open garden spaces. Of these blocks of buildings there are five one side and four and a smaller one on the other. The wards vary somewhat in size, but the average is 27·60 mètres long (90·5 E. F.), 8·13 wide (26·6 E. F.), and 5·10 high (16·7 E. F.), giving to each of the twenty-four beds which it contains a cubic space of about forty-seven mètres (1659 E. C. F.). There are two small chambers on each side of the entrance into the wards from the corridors, one for the attendant, the other for any special case; and at the further end of the ward there are two doors leading into small projections on the outside of the blocks, one being the lavatory and bath-room, the other the water-closet. At the end of the ward there is a large window, and the side windows have low sills, so that any one standing up can readily see into the gardens. The windows are made to open in swing divisions, so as to afford the means of ventilation. The piers between the windows are occupied by two beds each, and the wards are warmed by hot air admitted at the sides. The patients occupy only the ground and first floors. The whole arrangements of the hospital are extremely good; indeed, in cheerfulness the wards cannot be exceeded. The great objection is, however, that the separate blocks are placed much too near together. I estimated the space at certainly not more than thirty or forty feet.

At the back of the chapel, and communicating with both corridors on the first floor, there is a large room appropriated as a library and reading-room. There was a considerable number of books on the shelves and several papers were lying on the table; and in answer to my inquiry as to the class of books provided, and in proof of the absence of any narrow sectarian spirit in their selection, I was referred to shelves which I was told contained English works. At the back of the garden which occupies the rear of the hospital there is a lunatic asylum, provided with a small number of cells, and one or more of which are padded. At the time of my visit the establishment contained only one convalescent patient, and I was told that only acute cases are treated in the hospital; the others being removed to the colony of Gheel.

The establishment contains also a lying-in hospital, and on the premises there is a steam-mill for grinding the corn, and a bakery for preparing the bread used in this hospital and all the other public institutions in the city.

The other hospital to which I have referred is the Saint Pierre —like the Saint Jean, an old foundation; but the old buildings have not as yet been entirely removed, and are in process of being replaced by an entirely new hospital. The plan on which this is being constructed is entirely different from any which I have mentioned or which I have seen elsewhere. The new building will consist of two blocks, composed of separate wings radiating from a semicircular centre and corridor, with the object of affording to all parts of the building the greatest amount of exposure to sunlight and fresh air. The wards, which each occupy one wing, are only of small size, being constructed for the reception of from twelve to fourteen or sixteen beds; but the advantages anticipated by the special mode of construction seem to be much lessened by the free ends being cut off from the wards and occupied by small rooms—one the water-closet— and by a staircase. The patients are placed on the ground first and second floors, and there are at present two blocks of buildings of the kind described, one for men, the other for women. The space upon which the hospital is placed is very limited, and that may have necessitated the method of construction which has been adopted. Of its general advantages it would, however, be premature, in the very incomplete state of the works, to express any opinion. Dr. Van den Corput, who kindly showed me over the establishment, writes me that he is not able to procure any plan of the building, and that no account of it has as yet appeared in print.

From the brief description which has been given it will be seen that the hospitals visited in Northern Europe are all constructed on the same plan; and this remark applies, indeed, to all the modern hospitals in Germany and the adjacent countries, including the new hospital at Bremen and that of Rotterdam, the last of which I inspected some few years ago. This plan is that of a solid block of buildings, consisting of a front, with or without wings projecting at right angles to it behind, and so enclosing a larger or smaller extent of garden in the rear. The internal arrangements also are similar. The wards occupy the front of the building, and a corridor of communication is situated at the back, and the wards are placed so that one end, in which are the windows, is at the outer side, while the other opens by the door into the corridor. Sometimes there is a further communication by doors opening into the adjacent wards, and the beds are arranged against the blank sides. The wards are usually of different sizes, the larger for ten, twelve, fourteen, or sixteen patients, the smaller for one, two, three, or four. The central portion of the ground floor is generally occupied by the administrative and other offices of the establishments and the residences of the executive, and the patients are placed on the remaining portions of the same floor and on the first and second floors.

The space allotted to the beds in some of the hospitals is not so large as would in this country and in France be regarded as necessary. The Bethany at Berlin is, however, unexceptionable in this respect, the space allowed in it being equal to that in most of the general hospitals which have recently been constructed, either here or elsewhere in comparatively cold climates. I have already given the space allowed in some of the hospitals which I visited, and a table published by M. Husson affords us the means of instituting a comparison in this respect between various hospitals in this country, France, Belgium, and Germany (see table).

The wards are always warmed by the large porcelain stoves which are used in German houses, but in some hospitals arrangements are made by which the air in cold weather can be exposed to the heat of the stoves before being admitted into the wards; and in some of the newer hospitals the corridors, staircases, and other passages, are heated by hot water or steam. The windows are generally made to open by swing sashes or otherwise, so as to be employed for purposes of ventilation; but in some of the hospitals there are independent apertures for the entrance of air from without, and means by which a thorough draught may be made to pass through the wards by windows or other openings into the corridors behind, or by a system of tubes or canals passing to the tops of the buildings, and of which the currents are assisted by the heat of the smoke-vent or by the action of an aspirating machine or fan worked by steam-power.

The rooms for the nurses or other attendants are sometimes placed in the wards at the entrance from the corridors; in other instances they are situated between each pair of wards, and have separate windows looking to the outside and a door into the corridor, with other windows and doors into the wards to which they are attached. The former arrangement is very objectionable in all cases where the attendants have to be constantly in the wards, as it neither affords them a proper degree of privacy nor does it place them in favorable circumstances for the maintenance of health. When, however, the attendants are only in the wards when on duty and live in a separate part of the establishment, as is the case with the Protestant deaconesses or the Catholic Sisters of Charity, a mere cabinet in the ward may be all that is necessary.

The arrangements for the baths are generally very full and efficient in all the German hospitals; and generally much greater attention is paid to the use of baths of different kinds on the Continent than in this country. The Rotterdam hospital is, in this respect, a perfect model. Bath-rooms are usually placed either in the wards or within very easy access of them, and there are generally more complete sets of baths in some other parts of the establishment, for the use of patients who are capable of removal; while for such as can scarcely leave their beds movable baths are provided.

The closets are generally constructed on the English system, and are properly provided with water and trapped. The situation in which they are placed, however, varies. In some, and by far the larger number of the hospitals, they occupy a portion of the ward at the entrance from the corridor, where, also, the lavatory is generally placed. In others they are placed between two adjacent wards; and in the Copenhagen hospital alone are they situated entirely apart from the wards and on the opposite side of the corridor, outside the building—as is almost always the position adopted in the hospitals of this country. Either of the two former situations is objectionable, but the first much the most so, for with every attention to cleanliness there will always be some offensive smell from the closet, which must pollute the air of the wards or corridors adjacent to them; and in both cases they are in unpleasant juxtaposition to the ttendants' rooms.

The hospitals in Brussels are constructed on what may be termed in contradistinction to the plan mentioned, the French system. This consists in placing the wards in separate pavilions or blocks of buildings, surrounded on the two sides and ends by gardens, and only connected together and with the administrative offices, &c., by corridors opening into the other end. The Hospital Saint Jean affords a very admirable example of this method of construction. Indeed, though the better-known Lariboisière Hospital in Paris was constructed after the Brussels hospital, and very much upon the same plan, it may be doubted whether it is to be regarded as an improvement upon the earlier model.

In the Saint Jean the central garden is only enclosed by the hospital buildings on three sides, and is entirely open to large gardens on the fourth; and this must afford greater facility for free ventilation than where the court is entirely enclosed, as at Lariboisière. In the former hospital the administrative offices and the residences of the hospital staff, &c., occupy a court entirely separate from that upon which the wards abut, though with very ready access to all parts of the establishment; and this, too, is preferable to the arrangement at the Paris Hospital where there is only a common garden for the whole establishment. And, lastly, the arrangement of the wards in the Saint Jean is better; the water-closets, baths, and lavatories are placed on the outside of the blocks, and the ends of the wards are entirely open, so as to give to them a much purer atmosphere and greater appearance of brightness and cheerfulness than when the ends are closed and the closets placed in the wards, as at Lariboisière. In one respect the Brussels hospital compares disadvantageously with the Paris institution, the space between the separate blocks being more confined; but in both these hospitals the great defect is in this space, which is so limited that the atmosphere must always be damp and cold. The Saint Pierre Hospital at Brussels is at present so incomplete that the merits of its plan cannot be fairly estimated.

In considering the respective advantages of the two methods of construction which have been explained, I have no hesitation in giving the preference, for hospitals of large size, to that which may be termed the French system. The German plan offers, however, some advantages. It allows of the different parts of the institution being closely connected, so that the medical and economical management is easily accomplished, and the whole establishment is subjected to ready supervision and control. But it is also open to grave objections. The wards, having only windows at the one end and opening into the corridor at the other, must have only free ventilation through their centres, and the air entering them can have little direct access to the beds placed at the sides. On this system, also, there is no possibility of giving to any particular case or any special part of the ward, a greater amount of ventilation than to other parts; yet the opportunity of doing this is often a matter of the very greatest importance, and it can readily be accomplished in hospitals on the French construction, where the beds are placed in the piers between the windows.

In special hospitals for diseases of the chest, where extensive exercising corridors are needed in immediate connection with the wards or, what they virtually become in such institutions, the dormitories, the system of wards and corridors on opposite sides of a solid building seems unavoidable, for it would be difficult on any other construction to obtain the equality of temperature in different parts of the establishment which is desirable. This plan has, therefore, been adopted in the erection both of the Brompton and Victoria Park Hospitals. The situation of the beds at the sides of the wards and out of the current of air passing between the windows and doors is also, in such institutions, an advantage.

When, however, the German plan is adopted in the construction of a general hospital, it should, I think, be carried out in the mode adopted at the Cantonal Hospital at Geneva. In this establishment the wards are placed, not at right angles to the corridor, but parallel to it; and the windows on the outside of the building, both in front and behind, correspond both in situation and size with others in the wall between the wards and corridors. Thus, the air can pass directly through the building across the wards and corridors, and, as the beds are placed in the piers between the windows, they may be all fully ventilated, and a larger or smaller amount of air may be given to any particular part of the ward as required. This construction is similar, indeed, to that which has been adopted in the new wards at the London, Guy's, and King's College Hospitals, with, however, this further advantage, that there are only two instead of four rows of beds between the external walls. The objection to the plan would be the large amount of frontage required and the consequent expensiveness of the site for such a hospital.

In the construction of hospitals the great object to be attained is to obtain the freest exposure to sunlight and fresh air, with facility of communication between all parts of the establishment —in other words, to combine ventilation with compactness. A solid building certainly accomplishes the last requisite most readily, but the freedom of exposure and ventilation is greatest in a building on the pavilion system. In small hospitals a good and economical arrangement is to have the administrative offices and the officers' residences, hall, staircase, &c., in the centre, and the wards projecting from this on each side, without any corridor.

This plan has been followed in some of our older provincial hospitals, as the Derby Infirmary, and more satisfactorily—to judge from the published plans—in the hospital recently erected at Aylesbury. The older part of University College Hospital is also thus constructed, and the accommodation on the plan may be increased by making two or more wings intersect at the centre, either obliquely or at right angles, as in the hospital of San Luigi Gonzaga at Turin, or in one part of the Hôtel-Dieu at Lyons.

When, however, larger hospitals are required, the French or pavilion system of construction—of which we have the types in the Hôpital Saint Jean at Brussels, the Lariboisière at Paris, and the Herbert and Blackburn Hospitals in this country—is much to be preferred; and, I may add, it is the plan which will be adopted in the new St. Thomas's. The mode of carrying out this plan must necessarily vary according to the form and size of the piece of ground upon which the hospital is to be erected. In the Saint Jean and Lariboisière the blocks or pavilions are placed at right angles to corridors at the sides of oblong courts, and with these they communicate by their ends; in the Herbert Hospital the corridor is placed in the centre of the several blocks, which project on each side of it; and in the Blackburn Infirmary, while the corridor occupies a similar position, the pavilions are built alternately on the opposite sides. By this very ingenious construction the different sets of wards are separated, to double the distance which could otherwise be obtained. The piece of ground upon which the new St. Thomas's Hospital will be erected is a strip about 1700 feet long, extending from the south-west side of Westminster Bridge to near the Lambeth Bridge. Its width varies, rapidly narrowing towards the south or Lambeth Bridge end, but having an average of about 260 feet. In consequence of the length and narrowness of the site, it is therefore proposed that the hospital shall consist of six pavilions, placed parallel to each other and at right angles to the frontage of the site on the Thames. The communication between the several portions will be by a corridor placed at the back of the building. Such corridor will, however, only extend along the basement, ground floor and first floor; the second and third floors, for the buildings will be four stories high, being only connected together by staircases and lifts which will pass to the corridor end of each pavilion throughout all the floors. In point of free exposure, the new hospital will therefore have the superiority over all the others. It may, however, be doubted whether this advantage will not be gained at too great a cost, for the absence of a free communication throughout, at the level of each floor, will materially lessen the ready access to and egress from all parts of the establishment. The large space over which the hospital buildings must thus range,—for the extreme length from the centres of the two extreme blocks will be 860 feet,—will involve great expenditure of labour and time in furnishing the requisite supplies, and especially of food, &c. To obviate these objections I proposed to the treasurer to have only four blocks of building at right angles to the river; and to place other three, one to include the chapel and some of the residences, between the first series;—thus forming courts with buildings on three sides, and with ready communications at the angles by staircases and lifts, and throughout, at the same level, by a passage through the wards.

In the Belgian, Herbert, and Blackburn Hospitals there are only patients on two floors; at Lariboisière they occupy three, and at the new hospital a part of the ground and the first, second, and third floors will be appropriated to the wards; a portion of the ground floor will be employed for domestic purposes. This arrangement is one which, though the peculiarities of the site may have rendered it unavoidable, is to be regretted. The objections to a high building are not only that the access to the upper parts is tedious and laborious; but that the court-yards and gardens interposed between the blocks are in such cases too much shaded from the sun and air to be thoroughly dry, and for the plants and shrubs to thrive. In the new St. Thomas's the objection is less, as the high buildings will only be on two sides of each court, a third being only occupied by the low corridor and one of the officers' residences or the chapel, and the fourth being in great part free, except for an open colonnade.

In the construction of the modern German hospitals, preference has been shown for small wards. In the newly built French hospitals the wards, as in the old establishments, are generally large. In the St. Jean twenty-four beds are contained in the wards of ordinary size, and in Lariboisière there are thirty-two. The medical officers have been unanimous in recommending that the wards at St. Thomas's should contain about thirty beds; and it has been decided that each set of wards shall consist of a large room for twenty-eight beds, and a small one for two beds for cases requiring seclusion or special care. At the end of the ward next the corridor, staircase and lift, there will be a small room for the medical officers for consultations and special examinations, &c., a room for the nurses, a scullery or ward-kitchen, and the small special ward, which will be opposite the nurses' room and under constant observation. The main ward will be 120 ft. long, 28 ft. wide, and 15 ft. high; the special ward will be 16 ft.

by 14 ft., and 16 ft. high, thus giving 1800 cubic feet of space to each bed in the large ward, and 1840 to each bed in the smaller. The windows will be 3 ft. 6 in. wide, and the piers 4 ft. 6 in., and as only one bed will occupy each pier, the space between the beds will be five feet. With the object of enabling the patients, when seated, to look out of the windows, the sills are to be only 3 ft. high, and for purposes of ventilation the tops of the windows are to reach within a few inches of the ceiling. According to this arrangement there is reason to fear that the large amount of window space in proportion to the extent of blank wall, may make the wards too light and exposed to draughts. In Saint Jean and Lariboisière, in which there are two beds in each pier, the wards seemed to me sufficiently light; and the Herbert Hospital, where the beds are similarly arranged, the wards, when unoccupied, certainly appeared to have too much glare to be comfortable for patients seriously ill. It must also be a consideration whether, with so large a window space, the cooling will not be so rapid as to make it difficult to maintain an adequate temperature.

At the opposite end of the wards to the entrance there will be small external projections, in which the bath-rooms, lavatories and water-closets, will be placed; the latter will communicate with the wards by small vestibules, so as to prevent any effluvium escaping.

For purposes of warmth, each ward will be provided with three open fire-places; these will be placed in the middle of the ward, and the smoke-vents will pass direct from the fire to the ceiling. In the lower ward, where only one smoke-flue will be necessary, the space occupied by the fire will be only 2 ft. 6 in. each way, but in the upper floors a much larger space will be needed, amounting to 2 ft. 6 in. in width by 6 ft. in length. This mode of placing the fires will, I fear, detract much from the appearance of the wards. The stoves in the German hospitals appeared to me very unsightly, but perhaps in the larger wards at St. Thomas's the objection may be less. I also doubt whether, from the cooling of the air of the wards at the windows, there will not almost necessarily be draughts between the windows and fire-places. This plan has, however, been adopted in one or more of the newly erected provincial hospitals, and is said not found practically to be attended by the disadvantages named.

Besides the ventilation and warming afforded by the windows and open fires, it is proposed to introduce some additional method of warming the air before admission, and of removing the vitiated air. The last will probably be accomplished by upshot currents created by the heat of the smoke-vents or of the hot-water pipes for the baths and lavatories.

It requires some boldness, in the face of the ridicule which has been thrown on all systems of warming and ventilation, to advocate the introduction of anything of the kind into a new hospital. I do not, however, hesitate to express the opinion that in climates like our own, and still more in decidedly cold climates, some system of warming in addition to open fires, and of ventilation besides the change created by open windows, should be provided; so as to secure to the patients a suitable temperature and an ample amount of fresh air, when the weather is such that the windows cannot be safely kept open, and when the fires will not afford the requisite degree of warmth. If no such plan be adopted, one of two evils must almost necessarily result—either the windows will be kept open to the manifest injury of some of the patients, or they will be shut and the ward will not receive an adequate amount of fresh air. Any plan of warming and ventilation adopted should, however, be regarded as supplementary to the warming and ventilation by open fires and windows, and be had recourse to only occasionally and temporarily. I entirely disapprove of closing permanently the windows, and depending entirely upon the introduction of air previously warmed by some system of propulsion or exhaustion. The objections to various systems of warming and ventilation which have been proposed rather apply to their exclusive and permanent use, than to their employment in addition to other means and only occasionally. The closeness and unwholesomeness of wards in which the windows are not occasionally opened cannot be disputed; but, on the other hand, we have to guard against the introduction of cold air directly into a ward in all states of the atmosphere, and without reference to the different kinds of cases under treatment. We are all aware of the evils of defective ventilation, but every experienced hospital physician must also, I think, have met with instances in which patients have suffered seriously or even fatally, from exposure to cold draughts while under treatment.

At the end of each ward overlooking the Thames there will be enclosed verandahs, in which the patients too weak to go into the courts, will be able to sit or walk when the weather is suitable.

The front of the buildings will look on to the Thames, and will thus have a western aspect, but the separate blocks will be entirely open to the south. The courts, which, as before mentioned, will only be enclosed on two sides—the north and south—by the wards, will partially be open on the east side, and almost entirely so on the west. They will be of large size, the two side courts being 125 ft. by 110, the central one 200 ft. by 110. From the size and exposure, there is every reason to expect that the trees and shrubs will have sufficient sunlight and fresh air to thrive and form pleasant and cheerful gardens.

The residences for the medical and administrative officers will be placed on the east side of the two side courts, in connection with the corridor, and the chapel will occupy part of the central court. It would, however, I think, have been better that the residences should have been placed entirely away from the portions of the hospital grounds to which the patients have access.

The out-patients' department will occupy low buildings at the back of the hospital, with immediate access to the wide road in the rear. It will communicate with all parts of the main building by the corridor, staircases and lifts, and will be provided with large waiting-rooms, consulting-rooms for different classes of cases, dispensary, laboratory, and surgery, and a complete set of baths. The patients will enter the waiting-rooms by different doors from those by which they make their exit after having received their medicines, &c., and thus all crowding will be avoided.

The admission-rooms, both for in- and out-patients, will be distinct from the out-patients' waiting-rooms, and both will be provided with all the requisite offices.

The medical school will be placed in a building wholly apart from the rest of the establishment, at the Lambeth Bridge or south end of the hospital grounds. The administrative offices, with the hall and court-room, &c., will occupy a block of buildings at the side of the Westminster Road, thus affording ready access from without and to all parts of the establishment behind it.

The construction and arrangements of the different parts of the establishment are, I conceive, such as to afford every prospect that it will prove a good and efficient hospital. It is right also, in conclusion, to mention that, though I have not in every particular approved the arrangements, some of them which have been objected to are in accordance with the opinions of the majority of the medical staff, or have been adopted in consequence of the peculiar form of the piece of ground upon which the hospital is to be erected.

Table of the space allowed to each bed in different European Hospitals, chiefly derived from Husson's 'Etudes sur les Hôpitaux,' Paris, 1862, &c.

	Metrès Cubes.	Cubic Feet.
Berlin.—Bethany, small wards	50 to 60	1765·75 to 2118·9
„ large	30	1059·45
Charité	40 to 45	1412·6 to 1589·17
Jews'	24	847·56
Hamburg.—General Hospital	22·4	825·2
Jews' Hospital	26·6	940·8 [1]
Frankfort.—Heiligeist-spital	36·887 to 34·189	1206·8 to 1299
Munich.—Municipal Hospital	26·462 to 31·7	931·9 to 1119
Copenhagen.—Communal Hospital, large wards	30·9	1009 [2]
Rotterdam	34·	1200
Brussels.—Saint Jean	48·58 to 54·4	1715·8 to 1920 [3]
Paris.—Lariboisière	48·134 to 52·664	1699·8 to 1756·7
Blackburn	57·74	2038·7
King's College	54·88	1937·8
New St. Thomas's, large wards	50·96	1800 } [4]
„ small „	52·1	1840 }
London	48·13	1699·4
Guy's	46·7	1648·5
St. Mary's	42·4	1496·7
Westminster	31·14	1099·5
Victoria Park, new large wards	41·2 to 49·1	1457 to 1737·4 [5]
„ „ small „	38·8 to 46·26	1371 to 1635
Turin.—St. Luigi Gonzaga	96·9	3410·5
Milan	69·28	2446·2
Florence.—Santa Maria Nuova	61·19	2160·6

The English cubic foot is to the French mètre cube as 35·315 to 1.
The Hamburg foot linear is to the English foot as ·9408 to 1.
The Copenhagen foot linear is to the English foot as 1·0298 to 1.

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[1] Dr. Heilbut.

[2] Dr. Thelussen.

[3] Report.

[4] Dimensions given in paper.

[5] Report.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

CHRONICLE OF MICROLOGY.

By J. F. Streatfeild, F.R.C.S.,

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Part I.—Physiological Micrology.

Nerve-Plates of the Motor Fibres.—Their existence is re-affirmed by M. Kuhne in a note to the Paris Academy of Sciences. He goes on to say that all the parts of the nerve-plates, however complicated their forms may be, compose a distinct and complete organ, without any interruption. He is able to add that, only thus one sees the nerve-plate in the fresh and physiological state in which the muscle and nerve survives; that is to say, as long as excitability and irritability remain in the preparations. . . . . Like others, he has added osmic acid to microscopic preparations of muscular fibre provided with its nerve-cone of nerve-fibre, and of the terminal plate; and he has constantly seen a very marked black coloration of the nerve as far as the edge of the nerve-cone, whilst this, the plate in its interior and the contractile substance were but slightly yellowed. Therefore the nerve-plate contains no medullary substance, but should contain other, probably coagulable, matters, which produce its deformity some time after death.—*Archives Générales de Médecine*, December, 1865, p. 744.

Tooth Development.—Dr. W. Waldeyer, of Breslau University, at the close of a paper in which he details his protracted investigations on this subject, gives the following particular summary of the whole. He says—The teeth of man and mammals, inasmuch as they bear enamel, begin their development with the in-growth of the epithelium of the cavity of the mouth (Kölliker's *enamel germ*) in the maxillary blastema.

2. The enamel germ forms a continuous layer on the margin of the jaw, along its whole length (Kölliker).

3. At those spots in which teeth are to originate, papillary elevations of the blastema of the mucous membrane of the jaw grow

towards the enamel germ (*dentine germ*, tooth-bone germ, ivory germ) then consisting of a few separate cells with distinct dark nuclei, which tilts in before it the enamel germ.

4. The prepared tooth-origination consists of the tooth-bone germ and the enamel germ tilted in by it. The latter now has divided itself into as many individual larger masses as there are to be teeth; these separate divisions are now called *enamel organs*. Each enamel organ, as a cap, surrounds its dentine germ.

5. The enamel organ further divides itself into three parts, the *inner epithelium* or *enamel membrane*, the *outer epithelium*, and the *enamel pulp*.

6. The inner epithelium, the enamel membrane (Kölliker), consists of cylindrical nucleated cells, which are placed directly on the surface of the tooth-bone germ.

7. The outer epithelium is the direct continuation of the inner epithelium to the surface of the enamel organ, turned away from the dentine germ; it, through a narrow bridge, passes over the *neck of the enamel germ* into the epithelium of the oral cavity.

8. The enamel pulp fills up the space inclosed by the two epithelia and consists of star-like cells anastomosing together (*transformed epithelium*, Kölliker).

9. The neck of the enamel germ, in regard to the enamel organ, at a later period is destroyed by an extension of connective tissue of the tooth-flesh between them; the tooth-foundations are thereby separated altogether from the epithelium of the oral cavity. Remains of this enamel germ-neck here and their remain as small epithelial cell-nests (*glandulæ tartaricæ*, Serres, Kölliker).

10. The epithelium of the oral cavity, in the development of the teeth, is raised as a wall (*tooth-wall*, Kölliker) above the margin of the jaw. In the incisors it is raised out of the furrow between the lip and the margin of the jaw, and from this furrow also the enamel germ is tilted in *sideways*. In the grinders the latter extends from the edge of the jaw *straight downwards*; here also the tooth-wall accordingly is developed above in the jaw.

11. A little tooth-sac, so called, which, as a follicle, encloses the tooth-origination, does not exist; they are rather surrounded in their alveoli by a vascular mucous tissue, which is transformed later into connective tissue, and serves as a foundation for one portion of the jaw and of the tooth-flesh as well as for the cement.

12. The enamel has its origin in a direct calcification of the enamel cells (Schwann, Tomes).

13. The cross streaking of the enamel prisms are, perhaps, partly produced mechanically—by the, at first, mutual superposition of the still soft prisms crossing each other.

14. The crossings of the enamel prisms are explained by the circumstance that, while the enamel is forming, new enamel cells are constantly arising, and are, in various directions, inserted between the old ones.

15. The enamel cells are developed from the stratum intermedium, of a cell-layer analogous to the rete Malpighii of the epidermis close

under the inner and outer epithelium, lying between this and the star-shaped pulp-tissue.

16. The already existing enamel cells extend, moreover, by direct apposition of elements of the stratum intermedium.

17. The star-shaped enamel pulp, at a later stage, disappears; then both epithelia, the outer and inner, approach each other, the two fused strata intermedia, their common rete Malpighii in the midst.

18. At almost the same time the spongy vascular connective tissue which surrounds the tooth-originations in the alveoli drives vascular papillæ into the united epithelial layer, which, on its part, sinks down between the papillæ. (*Epithelial shoots,* Marcusen, Kölliker.)

19. After the formation of the enamel is completed, the remains of the epithelium is transformed into a laminar epithelium, of which the cells are finally fused into a kiud of homogeneous skin, whilst their nuclei disappear. *Enamel upper cuticle.*

20. The cuticle of Huxley is an artificial product; it represents the uppermost (most recent), least calcified layer of the enamel.

21. A "membrana preformativa" as a special formation does not exist.

22. On the upper surface of the dentine germ, by increase and enlargement of its cells, are formed the *ivory cells,* these are exactly analogous to the *osteoblasts* (Gegenbaur).

23. The dentine formation proceeds so that the cell-substance of the ivory cells themselves is calcified; but partially, however, whilst some of their processes, which partly are formed before the calcification, and partly only by partial calcification, are left as soft fibrous masses (*tooth-fibres,* Tomes, Kölliker).

24. The calcified mass of the dentine cells forms the, so-called, *intertubular substance* of the dentine; the cavities which receive the tooth fibres are the *tooth canaliculi,* which do not proceed into the enamel.

25. The inner surface of the tooth canaliculus facing the tooth-fibre is covered by a kind of elastic sheath (*tooth-sheath,* E. Neumann).

26. The dentine formation and the ossification process are completely analogous to each other.

27. The dentine cells are recruited by new formation and increase from the cells of the dentine germ which are placed, variously, in direct connection with the already complete dentine cells.

28. The cement consists of the transformed vascular mucous membrane of the alveoli; the process is the same as in the ossification, only that the cement is often formed by direct petrifaction of a fibrous connective tissue previously traceable of the alveolar periosteum. A special cement germ from which the cement of the crown would be formed (Robin and Majitot) does not exist; the formation of this cement is the same as that of the cement of the root.—*Zeitschrift für Rationelle Medicin,* series iii, vol. xxiv, pp. 209—12.

Spleen, &c. Minute Anatomy.—Professor Salisbury, in a long paper on this subject, says that "the arterial subdivisions all terminate in one of four ways, viz.—1. In *tubular glandules* followed by simple capillaries. 2. In tubular glandules followed by the oval splenic bodies. 3. In Malpighian bodies (rare) in which the internal coat subdivides into radiating capillaries. 4. In capillaries direct without the intervention of tubular glandules."—*American Journal of the Medical Sciences*, April, 1866, p. 314.

PART II.—PATHOLOGICAL MICROLOGY.

Blood-corpuscles in Rinderpest.—Dr. Andrew Smith of Edinburgh, in confirmation of previous observations, says—The blood examined was taken from the vessels of dying animals in the last stage of the worst form of the uncomplicated disease. In no other disease is the property of cohesion of the blood-corpuscles so marked and so difficult to overcome. The cells cling tenaciously to each other on the microscopic field and run together in irregular and shapeless masses, and their separation for the purpose of examination becomes a task of exceeding delicacy. The cell-walls of the blood-cells, which are at length by careful manipulation thus isolated, are seen corrugate, and after a time assume the remarkable stellar configuration represented.—*Medical Times and Gazette*, Dec. 23, 1865, p. 691.

Changes in the Blood in Marsh Fever.—Dr. Meigs of Pennsylvania relates three cases in which, after Frerichs, he found the peculiar pigment-granules described by him, On pricking the finger and placing a drop of blood under a quarter-inch lens it was seen that the red corpuscles were not more than one fourth as numerous as in health, the white corpuscles were about normal in quantity; but what especially arrested attention was the presence in the field of numerous minute particles of irregular shape, with angular edges, of blackish colour and entirely opaque. The spleen is congested and the red globules destroyed; the patient becoming anæmic. As the destruction of the globules proceeeds their red matter is converted into pigment, which pigment, in its various forms, is carried into the vascular apparatus of the body.—*American Journal of the Medical Sciences*, October, 1865, pp. 305—12.

Sclerosis of the Brain and Spinal Marrow.—W. Zenker of Gottingen contributes observations made by him during some months past on this subject. The microscopical investigation of sclerosed spots in the brain, made in a fresh condition, gave the following results.—Connective tissue-fibre existed in abundance, containing many interspersed nuclei, whilst no trace of brain-elements and nerve-fibres could be discovered. Moreover, amylaceous corpuscles, recognised as such by their reaction with iodine and concentrated sulphuric acid, were found scattered in great abundance and lying about in heaps in the field of vision, in size from that of a connective tissue-nucleus to that of a frog's blood-corpuscle, as round bright bodies, in which here and there a nucleus and also a concentric arrangement was distinguishable. As it seemed to be necessary to make a further investigation with finer instruments, some large sclerosed spots of the white substance of the cerebral hemisphere were hardened —a part in most highly rectified alcohol, and another part in a solution of 2 parts chromic acid to 1000 of water. After fourteen days the further examination was made and the following results obtained—

1. In the chromic acid preparation, sufficiently fine sections, made with a razor, showed a fine connective tissue, consisting of fibres. These appeared to be in part arranged as a network, in the meshes of which appeared the form-elements to be mentioned later. In thicker sections there was found only an apparently finely granulated mass, in which it appeared that many fibres were cut obliquely or across. This partly net-shaped arrangement of the fibres was not probably an artificial production by the coagulating influence of the chromic acid solution upon an albuminous fluid, because, in certain spots, the fibres united into thick bundles passed away parallel among each other. The same fibres, but less easily, could be shown in the alcohol preparations. After the addition of acetic acid the fibrous arrangement disappeared altogether.

2. In the alcohol preparations, but not in those made with chromic acid, were found those larger and smaller concentric corpuscles which, in a fresh condition, the iodine sulphuric acid reaction had exhibited.

3. In the chromic acid preparation there were found, moreover, in the meshes in question, very numerous oval, somewhat flattened, or egg-shaped and incurved nuclei. There could not positively be shown around the nuclei cell-processes and cell-membrane, although in carmine preparations the appearance of spindle-shaped, star-shaped, or anastomosing cells was often very evident. This was sometimes produced by the nuclei lying embedded in the meshes of the fibrous bands crossing each other. In no way could anything but nuclei be isolated. Fine sections of the alcohol preparation, moreover, were treated with the ammoniacal carmine solution, then washed out with a saturated solution of acetic acid in distilled water and concentrated glycerine; then, again, the covering glass being frequently lifted, it was washed with water and finally the preparation examined in a glycerine solution. By this method the nuclei described were found to be dyed red.

4. Also, by the treatment just mentioned, the capillary vessels appeared as distinct strings furnished with very numerous nuclei in their coats. As to the capillary vessels themselves, many of them showed considerable fatty degeneration of their coats, which was very beautifully seen by treating the alcohol or chromic acid preparations with diluted soda solution.

5. Independently of the sclerosed parts the rest of the brain showed no essential change. Ganglia-cells, however, were not unfrequently met with in the vicinity of the sclerosed spots. These were decidedly fatty degenerate, for they were almost completely untransparent, and filled with closely packed fatty granules; so also were their processes.

From all this taken together it results that, viewed microscopically, it was a pathological new formation of a tolerably distinct fibrous connective tissue, with overgrowth of its nuclei, and of the nuclei of the capillary vessels, with a probably secondary fatty degeneration of the coats of the latter. The ganglia-cells may then have been changed by pressure of the callous connective tissue.

The microscopical examination of the spinal cord, preserved in alcohol, gave the following results:—In the cervical region, sections of the posterior columns made with a razor, treated with an acetic acid solution showed the same characteristics as did the sclerosed spots of the brain; a network of fibrous bands (here even more pronounced than in the brain); abundantly interspersed nuclei; concentric corpuscles; fatty degenerate capillaries; and in the same proportions as in the brain.

Cross sections of the lateral columns showed neither connective tissue-fibre with nuclei, nor fatty degenerate capillaries, but, on the other hand, a large number of normal nerve-fibres, in some parts single concentric, and here and there brown stellate corpuscles resembling pigment cells.—*Zeitschrift für Rationelle Medicin*, series iii, vol. xxiv, p. 236.

Cataract Formation.—Dr. H. Kruse reports the examination of an extraordinary cataract of the eye of a patient of Professor Henle, in which all the other structures were normal. There had also been no visual defect. On a horizontal section all parts of the eye were in their ordinary position. But at the lower and back part the lens-capsule was found to be hollowed into a larger sac, whose envelope consisted of newly formed connective tissue. The upper portion of the lens and that behind the pupil was perfectly transparent and also showed nothing abnormal during the microscopic investigation. The eye was placed for several days in a solution of chromic acid (2 parts to 1000 of water). Within the sac, referred to, in the vicinity of the lens itself, were found, still recognisable, lens-fibres as follows: Individually the lens-fibres showed a curved cross-streaking and appeared as if they were divided by septula fan-wise. The whole resembled the compartments of an inlaid floor, excepting that here the divisions were irregularly arranged and made by curved lines instead of straight ones. Further removed from the lens there were found imbedded between the fibres small globular and larger angular cells, which modified in different ways the arrangement of the tissues. The globular cells had a distinctly contoured cell-membrane with granular contents, and a nucleus which also clearly showed a granular condition and occupied about a fifth part of the cell, whilst the larger angular cells showed wasted edges, but also possessed granular contents and nuclei. Besides these two chief types there were found many transitional stages from the globular to the angular cells, and in size also as well as form, so that it is fair to conclude that the angular form represents the final stages of development. As the cells became more frequent the trabecular supports of the changed lens-fibres seemed rarer, until at last there appeared an extensive branching network formed of threads intertwined. But this was not a question of threads and of sections of cell-membranes (analogous to the cell-tissue of the phanerogams), because, at the intersections of the threads—even where three threads met—there was constantly to be seen a round shining spot, which was easily confounded with the nucleus. By change of focus, however, it was shown that the apparent nucleus was a thread running perpendicularly to the level of the preparation and anastomosing with the others. The individual meshes were filled up with the cells just described, whilst at a greater distance from the lens itself the large angular cells increased and in some parts could hardly be recognised as such, rather forming an amorphous detritus mass. The width of the meshes was, on an average, 0·019 mm. Some were narrower and had a width of 0·008 mm.; on the other hand, others were 0·033 mm. in width; the threads were 0·001 mm. in thickness. The diameter of the angular cells amounted to 0·025, that of the globular to 0·015 mm. It is noteworthy that there were also individual spots in which there was to be seen a yellowish transparent homogeneous mass which was striped fibrously and formed cavities in which globular cells were imbedded. They were surrounded by normal lens-tissue and appeared as if they had been formed out of it.

An analogous case of senile cataract has been set forth by Wedl. In both cases probably the large round and angular cells have been produced by a division of the cells at the lens-capsule; the network from a partial resorption and condensation of the lens-fibres by loss of water.—*Zeitschrift für Rationelle Medicin*, series iii, vol. xxiv, pp. 261—3.

Diabetic Kidneys.—Among some pathological contributions of the late A. Erythropel, Krause communicates two similar cases. In the first there were found, in the cortical substance, numerous infiltrations of phosphate of lime, They formed masses lying in the tortuous canaliculi, indistinctly crystalline, strongly refracting light, almost cylindrical and of little extent longitudinally (0·1 mm). They were always near the Malpighian tufts, and, in some instances, it was apparent that the canaliculi involved were directly infiltrated beyond their inlaced neck by which they were connected with the capsules of the glomeruli. Otherwise, besides no inconsiderable hyperæmia, the kidneys seemed to be normal.

(Inasmuch as this patient had suffered from a rather severe inflammation of the glands and preputial skin, the secretion in the preputial sac was examined and many threads and spores of a fungus were found as in Liebreich's cases. They were mixed with cast-off epidermis-cells and many pus-corpuscles. In form they most resembled the Penicillium glaucum. The threads were 0·0028 to 0·0047 mm. in breadth; the oval spores, at most, were 0·0076 in length, and 0·0057 mm. in breadth.)

Krause continues—In a case recently observed, with regard to amyloid degeneration of the kidney and very accurately examined—

A woman, thirty years old, pneumonia of the right side, hypertrophy and dilatation of the right heart, relative insufficiency of the tricuspid valve, enlargement of the pulmonary artery, fatty, nutmeg, liver, amyloid degenerate spleen, fatty and amyloid degenerate kidneys. The microscopical examination of the latter showed that the glomeruli became of a blue-violet colour after an addition of iodine and sulphuric acid. So did also the afferent arteries and the beginnings of the urinary canaliculi, standing in connection with the Malpighian capsules. This colouring concerned the walls of the canaliculi, as might be made out in the sections, in which their lumen appeared transparent or of a yellowish brown colour. The great majority of the tortuous urinary canaliculi showed intense fatty transformation of their epithelial covering. In the boundary layer as continuations of the tubuli recti, there appeared bundles of (8 to 10) urinary canaliculi, very beautifully blue coloured by the treatment with iodine and sulphuric acid. The canaliculi of Henle were unchanged.—*Zeitschrift für Rationelle Medicin*, series iii, vol. xxiv, p. 221, &c.

Reticulated Hypertrophy of the Gastric Mucous Membrane.—Dr. Ebstein, of Breslau, reports this case, in a man, aged nineteen, who died of purulent infection after an abscess, deep seated in the thigh, and whose digestive functions had never been disturbed. The peculiar appearance only existed in a part of the gastric mucous membrane. Its surface was there overrun by a considerable number of leaf-like elevations, crossing in different directions and incapable of being unfolded. This was, besides, quite unlike the papillary structure which was elsewhere perfectly exhibited. The lamellæ were about 1 millimètre in height, 2 to 5 millimètres in length, and 0·5 to 2 millimètres in thickness. The author dried the parts after having treated them for a few instants with one third of concentrated acetic acid and water. They were then easily cut in fine slices, some of which were then soaked in carmine solution.

The muscular tissue and the sub-mucous layer showed nothing particular. The foliated prominences were at their surface formed by the normal covering of cylindrical epithelial cells, then by the layer of tubular glands, and lastly, at their centre, by leaf-like prolongations of the layer of smooth muscular fibres which separates the mucous from the sub-mucous tissue. This layer was notably hypertrophied; it also showed in the insterstices of the tubular glands a great many prolongations in which the muscular fibres were mixed with a good deal of connective tissue.

Dr. Ebstein concludes that this was a peculiar congenital conformation; for he says that nothing at all like the above-described disposition of this muscular layer exists in the normal state, and that neither was there any evidence of disease having existed in the mucous membrane, nor of any disturbance of the functions of the stomach.—*Archives Générales de Médecine*, September, 1865, pp. 349—50

Cystic or Hydatiniform Disease of the Chorion.—Dr. Braxton Hicks has expressed his disbelief in the existence of a class of so-called "proliferous cysts." Those of the chorion he considers not as growth of cysts from cysts, but merely a subsequent change in parts already formed or forming at the time the change commenced in the large cysts to which they were attached. He thus describes four specimens lately examined by him, and with similar results.—Whatever may be exactly the tissue involved in the change, whether the cells from which are formed the blood-vessels, or those supposed to perform the glandular office, the *first difference* which is apparent, coincident with the enlargement of a portion of the villus, is the filling of it throughout by either complete cells, or by some in an incomplete state. These entirely fill up the interior, thereby rendering the appearance of the dilated part opaque. The form and size of these cells are variable, some being four or five times larger than the others, of a globular or oval shape; the walls sometimes of very considerable tenacity, with or without nuclei. Some are branching connective tissue-nuclei (unformed or germinal matter of Beale), either distributed in a linear manner, or irregularly dispersed, in variable states of formation, connected together frequently (especially at a later period) in a stellate manner. One can scarcely doubt but that it is the growth of these elements which distends the villus at the commencement of the change, and, indeed, to a considerable period. However, after a time, a clear fluid infiltrates between the cells, &c., causing their separation one from another (particularly the more delicate kinds), while about the same time many of the cells undergo dissolution, probably by the ordinary processes. But also the germinal matter more completely matures itself into connective tissue, most commonly of the stellate variety, which is to be found in the later stages of the disease in considerable quantity; not in the incomplete state found at first. It happens occasionally, but by no means very frequently, that the centres of this connective tissue undergo fatty degeneration. In such cases, of course, ultimately all the connective tissue disappears, nothing but clear fluid remaining. With regard to the walls of the cyst, I found it to be composed of two layers, both having for its basis a transparent delicate membrane. In the external one were to be seen nuclear masses, which at first were far apart, but afterwards closely approximated, and had small granules (fat ?) within these, thereby rendering the cyst opaque. The internal layer possessed fewer nuclear masses, generally of an oval or elongated form.

The nature of the small club-shaped projections on the surface of a large cyst can be seen, by comparison with the normal villus, to be the same in the mode of formation, nor does there seem to be any growth of them from the villus subsequently to its cystic change.—*Guy's Hospital Reports,* 1865, pp. 182—3.

Molluscum Contagiosum.—Professor Virchow distinguishes three affections which have been hitherto confounded under this head, and defines the nature of that to which alone contagiosity is attributable, and which is called by him *Epithelioma molluscum*. He points out that this quality has never been allowed to the atheromatous tumours, sebaceous cysts, or cholesteatomata, which are of an epithelial nature, and formed by retention and obliteration of the orifice, and surrounded by a capsule of connective tissue of new formation; this he calls *Atheroma fibromatodes* or *Atheroma molluscum*. The true *Molluscum contagiosum* is neither a wen nor a cyst, but an epithelial tumour, with an orifice communicating with the skin-surface. Its contents can be at least partly expressed—as a milky liquid or a cheesy jelly. In section they resemble a lobulated gland, and, more accurately observed, two distinct portions are seen—a soft substance, filling the canals, and expressible and a compacter, more resistant matter, forming the walls of these cavities. The latter is formed by regular layers of cylindrical cells, which is the, much developed, Malpighian layer. The soft substance is made up of epithelial cells and corpuscles analogous to fat. The epithelial cells are flat, polygonal, and, for the most part, non-nucleated, yet one can make out, hardly here and there, one containing granulations and its nucleus, but depressions are to be seen on these cells, one only, or even three or four, and in them are lodged corpuscles of a peculiar nature. (One never sees those fatty granulations and those little drops of fat to be seen in sebaceous glands.) These corpuscles most resemble swollen starch-grains, but they do not give the characteristic reaction with iodine, with addition of sulphuric acid. They appear as little homogeneous bodies, smooth, with a sharply defined outline, with neither membrane nor nucleus perceptible. They have been recognised as "peculiar bodies" by English observers. M. Virchow has not been able to recognise in them the least trace of evolution or of an ulterior development. He admits, resting on observations made by him on other epithelial tumours, that these bodies are due to a peculiar degeneration of epithelial cells. It is, then, simply hyperplastic, the starting point, according to the author, being the hair follicles, an affection of a benign nature and is to be considered as a hyperplastic epithelioma—*Archives Générales de Médecine*, November, 1865, pp. 609—11.

Cysto-sarcoma of the Thyroid Gland, with calcified fasciculi of connective tissue.—Dr. F. Thörl, of Göttingen, describes this morbid growth, which was found in a woman forty-six years old. In general appearance it had not the remotest resembance to the thyroid gland. It was an irregular body of considerable size, of many overlappings, and here and there with cysts interspersed. Its great length was twelve centimètres; thickness nine and a half centimètres. The author says:

"As to its microscopical characteristics, the tumour must be considered generally as one of the so-called cysto-sarcomata, if one would call by that name all those tumours which for the most part consist of a more or less fibrous and vascular connective tissue mass in which numerous cysts are embedded. Preparations from the most different parts of the tumour showed the microscopical appearance of connective tissue. By treatment with distilled water the compact, fine, undulatory course of the connective tissue fibres with their intervening spaces, and their groups of mostly long, spindle-shaped or star-shaped forms, was unmistakeable. Near the connective tissue fibres were found at one part, also elastic fibres, marked by their great brilliancy, when the preparation had been treated with acetic acid, by which the streaking of the connective tissue appeared, and the fibres alluded to could be still more plainly seen.

"The cyst-walls were also composed of connective tissue, with interspersed elastic fibres. The cyst contents were composed of blood-corpuscles and coagulated fibrin. There were to be remarked especially individual cells of polygonal shape, which appeared to be separated epithelium of the inner cyst-wall. The capsule of the tumour generally was altogether composed of larger fasciculi of connective tissue.

"At some little isolated spots the uniformity of the finer structure of the tumour underwent considerable changes. The spots were found at the margin, of a remarkable hardness, of lustrous white appearance, and they seemed, by their fragility, to be connected with calcareous concretions. In the first place, the preparations which had been obtained from these spots were treated with distilled water. The appearance then presented by these preparations bore little or no resemblance to that of those taken from other parts of the tumour. Cylindrical bundles of remarkably bright lustre, arranged confusedly, of serpentine course, with dendritic ramifications, defined with dark contours against the neighbouring tissue. Their diameter was not inconsiderable, and varied from 0·025 to 0·030 millimètres. Besides a longitudinal streaking of the bundle, there was an unmistakeable cross-streaking. Upon or in them, as in the neighbouring tissue, were enclosed numerous little dark nucleoli.

"In the next place, the same preparation was examined further with use of hydrochloric acid. The enclosed, supposed to be simple, nucleoli disappeared with development of gas: the whole preparation, moreover, became of a brighter, more transparent appearance. The many ramifications and anastomoses of the bundles, their varied confusion appeared yet more plainly, but the lustre had disappeared. The longitudinal streaking was also much less plain, indeed almost invisible, whilst there appeared a very characteristic and undoubted cross-streaking of the bundles. After this, the hydrochloric acid was carefully washed out of the preparation, and it was soaked in a solution of iodine-iodide of potassium. In this way the bundles of which we have been speaking, like all azotised tissues, were coloured yellowish-brown. The experiments were slightly varied, and the examination made again and again, with the same results.

"When the chemical and physical natures of the bundles have been thus shown, the question is, the signification of the result. Possibly one might consider them to be vessels; if so, there must have been albumen in the cross-section of the bundle, but this is decidedly contraindicated; or it may be looked upon as a simple chalky concretion, perhaps as a calcified colloid mass. Their solidity, and the escape of gas on the addition of hydrochloric acid, can only lead to a conclusion, whilst the remarkable organisation of the bundles, the manifest longitudinal and transverse striping of the fibres, is directly opposed to this; but especially, and before all things, the evident fibrillation at the torn-off ends, which was distinguishable through the whole thickness of the bundle. This plainly exhibited texture of the ground substance, and the yellow-brown colouring on addition of iodine appears to me decidedly to prove its connective tissue nature; we have here, no doubt, to do with reticularly arranged connective tissue bundles, which are enclosed by elastic fibres. Is, now, this bundled or trabecular disposition of the connective tissue with encircling fibres not frequent, so it is much seldomer found that these, as in this case, calcify and are found completely infiltrated of carbonate of lime? Even from the strong lustre of the bundle treated with distilled water one cannot safely conclude that this is the case, but especially from the escape of carbonic acid.

"Although it is known that connective tissue bundles may be calcified, I have never known a case analogous to the above, with reticular anastomosing calcified trabeculæ in tumour of the thyroid gland."—*Zeitschrift fur Rationelle Médecin*, series iii, vol. xxvi, p. 180.

Onychomycosis.—In his remarks on two cases of this disease, Dr. Purser comes to the conclusion that epiphytic diseases are the effects of really vegetable organisms rather than of perverted development in the cells of the part. The parasite, once having taken root, becomes the principal cause of the local disease which it serves to keep up and aggravate. In both cases the alteration in the nail commenced at the root and extended downwards. The nails were much thickened, showed no lunulæ, and beneath them there was found abundance of loose cells, and among them, and especially in the deeper layers of the nail, a fungous growth. 1. *Spores*, circular or oval, either scattered, collected in groups, or forming moniliform chains. In some of them a central nucleus-like spore was apparent. 2. *Tubular filaments*, tortuous and branching: these were for the most part jointed at intervals, and many of them contained small shining bodies. 3. *Larger, less branched filaments*, of brownish colour, and containing spores at regular and close intervals; the walls of these filaments were sometimes indistinct, the spores being apparently attached to each other, end to end, forming a moniliform chain, which was often seen to terminate in a dense cluster of minute spores, or in a mass of granular matter. 4. *Granular matter.* All these were mixed up with tolerably healthy nail plates, and were rendered very clear by caustic soda or potash. The patient, in the second case, suffered from a secondary syphilitic eruption. The microscopic appearances only slightly differed from those above given.—*Dublin Quarterly Journal of Medical Science*, November, 1865, pp. 353—61.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

On the Treatment of Wounds by Ventilation. By M. **Bérenger-Féraud.**—In this paper, M. Bérenger-Féraud draws attention to a mode of treating wounds and ulcers described by M. Bouisson of Montpellier in the second volume of his *Tribut à la Chirurgie*, under the name of ventilation. This consists in fact in leaving small wounds exposed to the air, and in acting upon larger ones by means of the domestic bellows for a period varying from five to twenty minutes every two, three, or four hours, according to the amount of discharge and moisture that may be present. The object is to secure the formation of a crust over the surface of the wound, under which cicatrization takes place far more rapidly than when the surface is not so protected; and the applications must be sufficiently frequent and prolonged to maintain this crust of a certain thickness. When the crust acquires a degree of rigidity, however, it must be displaced and another formed; and when the discharge is very abundant, the alcoholic dressings, now so much in vogue in the Paris hospitals, should for a while precede the ventilation. The influence of this last in improving the condition of the wounds is almost immediate, a disposition to cicatrize and a diminution of the discharge soon being apparent.

This mode of treatment, according to its originator, M. Bouisson, may determine sedative, astringent, siccative, antiseptic, and tonic action; but it is by no means indicated in all kinds of wounds, and especially in those whose depth is great in proportion to their superficial extent. Thus, it is not fitted for penetrating wounds, as those of a fistulous character, or characterised by anfractuosities. Abundant suppuration is a further contra-indication, except, indeed, when this is due to a mere hyper-secretion dependent upon local or general atony or perverted nutrition, and to the lessening of which alcoholic dressings supply a useful preliminary to the employment of ventilation. In slight burns other means may be preferable, as of more convenient application; but in those of the second and third degree, arrived at the stage of a simple denuded wound, ventilation may advantageously supersede cotton and other impermeable applications. In resorting to this means for ulcers, we have to attend to the constitutional cause of these, as well as to render them by various local applications apt for cicatrization before we resort to ventilation.

Among the secondary advantages of this mode of treatment may be mentioned its simplicity, its easy applicability by the patient or his friends, its economy and its cleanliness. It substitutes a dry for a moist surface, diminishes the chances of septic decomposition, and lessens the chances of infection of the surrounding atmosphere. —*Bulletin de Thérapeutique*, Jan. 31, Feb. 15.

On Cirsoid Aneurisms of the Limbs. By M. COCTEAU.—M. Cocteau terminates a memoir upon arterial varices, or cirsoid aneurisms of the limbs, with the following conclusions:—1. Arterial varix is not necessarily preceded by an erectile tumour. 2. The arterial walls are generally thinned, but they are sometimes found hypertrophied. 3. The veins present analogous changes. They are always dilated; their coats being sometimes thinned, and sometimes thickened. 4. The symptoms are the same as those of aneurismal varix. 5. Cirsoid aneurism has some signs in common with arterial aneurism; but essentially differs from it, inasmuch as the blood it contains has no tendency to coagulate. 6. The modes of treatment adopted for arterial aneurism present in these cases no chance of success.—*Archives Générales*, December.

On Chronic Urethritis. By M. ALLAIRE.—M. Allaire terminates an essay upon this subject as follows:—1. It is almost always the result of one or more blenorrhagies. 2. In 90 times out of 100 (in 99 according to Delpesh) one or more strictures exist in chronic urethritis. 3. There is a tendency to the formation of stricture after acute urethritis—60 times out of 69 cases, according to M. Marchal. 4. Changes in the surface of the canal are rare, those of the substanec of its tissues being much more common. 5. The formation of the fibrous tissue being slow, treatment by dilatation has the better chance of success the sooner it is employed after such transformation and the atrophy of the normal tissues. 6. The seat of stricture is generally in front of the membranous portion. 7. In the great majority of cases cauterization, even when superficial, urethrotomy, permanent dilatation, and forced dilatation, should be rejected. 8. Rapidly repeated dilatation (*coup-sur-coup*) should only be employed where the strictures are easily dilated, and when the canal presents but little sensibility. 9. Gradual and temporary dilatation should be almost exclusively resorted to, and under its employment accidents either do not take place at all or are very rare and of short duration. 10. Strictures may be radically cured. 11. To compass this the normal calibre of the urethra must be exaggerated, and sometimes the meatus requires to be divided. 12. Bougies must be introduced from time to time during several weeks. 13. A bougie must be passed once a day, and immediately withdrawn, increasing the size progressively from one sixth to one half a millimètre. The bougie must be passed in gently, so as to give as little pain as possible; and when there is strong contractility manifested by the stricture we should pause for a few seconds, and press with the end of the bougie, which will then pass much more easily. It is often of great importance to pass the instrument while the patient is in a bath. 15. The bladder should be penetrated into, or the stricture passed only to the extent of two centimètres. 16. When any accident arises—such as febrile paroxysms, cystitis, profuse discharge, articular pains, loss of semen, &c.—the dilatation should be suspended. The flow of blood at the commencement does not call for this precaution. 17. Baths or sitting baths should be employed after each *séance*, especially at first. 18. Copaiba, injections, &c., should be abstained from. 19. A metallic instrument should be introduced for several days at the end of the treatment. 20. The duration of the treatment varies greatly; but while a case may be sometimes cured in from eight to twenty days, as a general rule, a month or six weeks is required. 21. When No. 14 or 15 bougie (equivalent to eight and eight and a half millimètres) can be introduced, the discharge will ordinarily stop of itself in the course of a fortnight, without the use of injections.—*Recueil de Mémoires de Médecine et de Chirurgie Militaires*, December.

Treatment of Varices and Varicose Ulcers.—Dr. Faure states that his object is not to give any general account of this subject, but merely to describe a mode of treatment which he has found successful. Stated in general terms, it consists, in the case of varices, in the interception of the course of the blood in the affected veins, and in that of the ulcers in isolating them from the integument. Not having found Brodie's operation for varices succeed in his hands, he resorted to the plan of dividing the varix and the skin transversely, and, after dissecting the edges of the section, placing slips of agaric between them in order to prevent all approximation of the orifices of the vein. This division of the principal veins induces at once the subsidence of the most voluminous varices, and is followed by no ill consequence. With respect to varicose ulcers, while many of them are amenable to rest combined with very simple means, there are some which obstinately resist, or even become aggravated by, all ordinary treatment. In bad ulcers of this kind two indications have to be fulfilled.

" To cause a modification in the tissues concerned and put an end to the retraction which opposes the approximation of the edges of the ulcer. This double condition is fulfilled by one of the simplest of operations. It consists in circumscribing the ulcer, together with a certain extent of the surrounding tissues, between two curved incisions, joining each other at their extremities and extending through all the thickness of the integument. Their edges are to be dissected so as to detach from the subjacent parts all that surrounds the ulcer. In this way each of the veins is divided throughout, and the ulcer remains in an islet of tissues isolated at its edges from the rest of the integuments. A great change is thus operated in the circulation of the ulcerated parts, and, relieved from all traction, there is nothing which prevents them yielding to their own contractile force and cicatrizing. The result of the operation is immediately manifested, for the incisions, at once separating, sometimes acquire a centimètre in breadth, while the part affected, from being red, tumified, and tense, becomes pale, soft, and flexible. The ulcer, by the very next day, loses much of its extent, its edges becoming effaced, &c."—*Archives Générales*, March.

[In relation to this class of affections, we may notice a successful case of varicocele recently (*Gaz. des Hôp.*, No. 64) treated by the galvano-caustic by M. Amussat.]

On Umbilical Hernia.—M. Demarquay calls attention to the fact of how little attention has been paid to perfecting our procedures in respect to umbilical hernia as compared with the inguinal and femoral forms of the malady. The inefficient manner in which it is supported in children cannot have escaped the observation of every one, and, with few exceptions, matters are still worse with the adult, giving rise, not only to the danger which ensues on strangulation, but also to a variety of gastro-intestinal disturbances. These last at once disappear when a suitable apparatus is applied. By far the best form of this that M. Demarquay has employed consists in a little pyramidical pad formed of very supple vulcanized caoutchouc, and filled with air. The hernia in children can, by the aid of this, be easily retained. When it has to be applied it is affixed to a very supple strip of diachylon, as broad as itself and nearly long enough to surround the body. It should be renewed every day or two after placing the child in a bath, but it can be dried again for future use, and is very cheap. When at the end of some months the hernia has become very small, or has no longer a tendency to come out, the caoutchouc may be replaced by a little ball of wadding. These air caoutchouc pads may also, when properly adapted in size and form, be very advantageously employed in the adult, affording complete relief to the distressing symptoms induced when the hernia is ill-supported. The diachylon strip requires to be proportionally broader, and two smaller strips should lap over it above and below the pad, to keep it *in situ.*

When umbilical hernia becomes strangulated it is a very serious occurrence, most of the patients dying of peritonitis when an operation has been performed for the relief of the strangalation. This has induced M. Demarquay to consider whether the operation without opening the sac, so often successfully performed for inguinal hernia, should not be preferred. It seems, indeed, pre-eminently suited for this form of hernia, for, first, the ordinary operation, leading to so large an exposure of the abdominal cavity, is usually fatal; and, next, the course of a strangulated umbilical hernia is much slower, with respect to the production of sphacelus, than is the case in inguinal and femoral hernia, there being, in fact, less danger of the intestine becoming divided or ulcerated by contact with the ring. For these reasons, M. Demarquay quite recently performed the operation in this manner. An incision was carried upwards and outwards from the summit of the tumour, which equalled a large orange in size. Beyond the base of the tumour the incision only implicated the skin. At the base of the hernia, the external portion of the sac having been recognised, a very small puncture was practised in it, through which the end of the finger was passed and the intestine pushed back. This effected, a curved grooved canula was introduced between the intestine and the umbilical ring, the strangulation being relieved by means of an incision of about a centimètre in length made with a falciform bistoury, directed upwards and outwards. The intestines were left *in situ*, no attempt at their reduction being made. The wound in the skin was brought together by the interrupted suture, and opium was administered in divided doses. For the first twenty-four hours all went on well, when renewed symptoms of strangulation appeared and the patient died. "I found that the cause of death was due to a secondary strangulation, which had been produced by the swelling of all the parts, consequent upon the inflammatory process set up within the cavity of the sac; and I asked myself whether I had not been the involuntary cause of death by not dividing the stricture to a greater extent. I believe this to be the condition of success, and on the next occasion I shall resort to the same procedure, and relieve the strangulation with less timidity. It may not seem very logical that I should recommend an operation which has failed in my hands, but I may observe that, in presence of the inefficiency of our present means of treating strangulated umbilical hernia, it is very desirable that a more efficacious procedure should be sought for, and that in operative medicine we cannot judge of the utility of a procedure from a single case. An operation, however badly conceived and executed, may sometimes succeed, while a rational one fails. Success is a great thing in surgery, but we must not disdain the investigation of new applications of rational measures when these have for their aim the treatment of an affection almost constantly fatal."—*Union Médicale*, No. 33.

Treatment of Anthrax.—Dr. Larghi, of Vercelli, describes the form of treatment which during the last twenty years he has pursued with great success in the treatment of anthrax. As soon as he is called to a case—and the sooner the better—he makes a free crucial incision, so as to reach the sound parts at the margin of the tumour, as well as through the depth of its substance, and then proceeds to freely apply the solid nitrate of silver, sticks of which he has ready mounted on an elastic catheter. Every portion of the incised parts, as well as any spontaneous opening that may have taken place, are thus thoroughly cauterized with the nitrate—which, in fact, is thoroughly dissolved, while when the tumour is very deep a second cylinder is applied. A pultaceous semi-liquid mass results, and on this being removed any points whence blood issues are again cauterized. The edges of the wound are also carefully cauterized. The incisions and cauterizations are rapidly performed, and the pain caused is not durable, while a calm sleep soon supervenes. The tumefaction and pain of the anthrax rapidly subside, and the separation of the eschar is allowed to take place spontaneously, a weak solution of nitrate of silver only being thrown into the cavity. Neither erysipelas nor purulent absorption ensues, the fever ceases, and the patient rapidly recovers.—*Annali Univ. di Med.*, April, p. 127.

On the Employment of Anæsthetic Agents during Operations on the Eye.—In this paper, M. Wecker considers three points:—1. What are the real inconveniences in the employment of anæsthetics in ocular surgery? 2. What are the operations in which such employment is useful and legitimate? 3. Which is the anæsthetic agent that should be preferred? With respect to the inconveniences attendant upon the employment of anæsthetics, M. Wecker agrees with Professor Jacobson, of Königsberg, in the belief that most of these are due to the timidity with which they are administered, large doses capable of producing absolute muscular resolution being necessary in delicate operations on the eye. Still, he observes, it must be remembered that Jacobson practises in a region where, the use of alcoholic drinks being much more general, larger quantities of chloroform are admissible than they would be in warmer or more temperate climates. The production of obstinate vomiting, either during or immediately after the operation, has been attributed to the employment of chloroform; but, as Jacobson truly observes, such vomiting is never so sudden in its appearance as to take the operator by surprise, and that the eye may always be protected from its injurious influence by supporting it with a little charpie held in the hollow of the hand. The compressing bandage, the use of which is indispensable after most operations on the eye, sufficiently protects the organ during any efforts at vomiting which may then take place. To say the least, the objections against the use of anæsthetics have been exaggerated. Among the operations for which they are especially indicated are those in which all violent contraction of the muscles of the eye and eyelids may be the cause of immediate accidents, in which the globe of the eye is largely opened. The flap operation for extraction of cataract here occupies the first place; and it is necessary to have performed a great number of extractions, both with the aid of and without the anæsthetic, in order to be able to appreciate how much, in the former case, the section of the flap is facilitated, and how much less trouble the operator experiences in giving to it the exact size and position. And if these are not considered as sufficient inducements to resort to anæsthetics, they will still be found valuable auxiliaries at the moment when it is sought to give issue to the cataract; for, in a subject who has been completely anæsthized, and when the flap in the cornea has been sufficiently extensive, prolapsus of the vitreous humour, even to the slightest extent, may be prevented. Of this M. Wecker has fully assured himself by having in a great number of cases been able to effect extraction of the lens without opening the capsule, by merely sliding a curette under it, and without any loss of the humour. In fact, when all the muscles of the eye are in a condition of relaxation, and when, after the incision has been made, all intra-ocular pressure has ceased, the vitreous body has no tendency to escape, anæsthetics thus reducing to nothing one of the chief dangers of this operation. Thanks to them, also, all operations become practicable on the youngest children, rendering superfluous the various complicated apparatus which have been invented to fix the head in subjects of this age. We have no longer to dread, as heretofore, that the cries of these little patients will induce hernia of the iris, if, before the anæsthetic influence has passed off, we apply over the eye a sufficiently firm compress. Of course, anæsthetics are just as much indicated in painful operations on the eyelids, enucleation of the eyeball, &c., as in most other surgical operations.

As in operations which themselves cause no danger to life the safest anæsthetics are the most eligible, M. Wecker gives the decided preference to chemically pure rectified ether at 60°, which, indeed, for some time past, he has exclusively employed. He regards the objections to this—that it requires more time to produce anæsthesia, and never renders it complete as quite unfounded—when the article employed is pure, and is from the first inhaled in very large quantities. The ether he employs comes direct from New York, and the patient is encouraged to inhale it as deeply as possible, disregarding the slight cough that is at first produced. The effect is produced almost as rapidly as with chloroform, while the resolution is so complete that extraction without opening the capsule can be readily performed. Moreover, the surgeon proceeds with an amount of security he can never possess when chloroform is employed to the extent of producing the complete resolution that is necessary for operations on the eye. When chloroform is employed, moreover, the inhalations have to be interrupted at the various stages of the operations, while with ether they may be proceeded with continuously to the very end, avoiding the sudden rousing up, and the dangerous irregular movements which may follow. Ether, then, should be exclusively employed in operations upon the eye.—*Bulletin de Thérapeutique*, May 30.

Summary.

Amputation.—Szymanowski. Justification of Gritti's Operation. (Prag. Viertelj., Band ii. A defence of amputation through the femoral condyles, which has now been resorted to about twenty-seven times, but with indifferent success.)—Syme. Amputation of the Knee. (Edinb. Journ., April.)—Zeis. Estimate of Amputation at the Knee-joint. (Langenbeck's Archiv, B. vii, H. 3.)—Zeis. Case of Transplantation of Skin to cover an exposed Stump. (Ibid.)

Aneurism.—Duncan. Galvano-puncture of Aneurism. (Edinb. Journ., April.)

Bone.—Guersant. Necrosis and Caries in Children. (Bull. de Thérap., April 30.)

Cæsarean Section.—Wasseige. Case of Successful Cæsarean Section. (Presse Méd. Belge, No. 18. Intestine became strangulated in the wound of the uterus, giving rise to artificial anus, which healed spontaneously.)

Cancroid.—L. Meyer. Clinical Observations on Cancroid of the Female External Genitals. (Virchow's Archiv, April.)

Dislocations.—Verneuil. On Reduction of Old Dislocations. (Bull. de la Soc. de Chirurgie, tome vi, pp. 492—513. With a discussion.)—Streubel. Mechanism of Dislocation of the Patella. (Schmidt's Jahrb., H. 3, p. 311.)—Howe. Case of Diastasis of the Sternum. (New York Med. Journ., April.)

Ear.—Wendt. Affections of the Cavity of the Tympanum in Children. (Küchenmeister's Zeitschrift, H. 2.)—Lucæ. Apparatus for the Propulsion of Air into the Ear. (Deutsche Klinik, No. 8. With figures.)—Schwartze. On Subjective Noises in the Ears. (Berlin Klin. Woch., Nos. 12 and 13.)—Roosa. Bony Growths in the Meatus Externus. (New York Med. Journ., March.) — Lucæ. Peculiar Formation in the Membranous Semicircular Canals. (Virchow's Archiv, April.)

Eye.—Becker. Report on Prof. Arlt's Eye Clinic for 1863-65. (Med. Jahrb. der Gesell. der Aerzte in Wien, Heft 3.)—Houdin. Effects produced with the Iridoscope. (Gazette Méd., No. 13.)—Cohn. On Myopia in School-children. (Deutsche Klinik, No. 7. A statistical investigation as to the dependence of its frequency on the school-desks and amount of illumination.)—Hirschler. Case of Saturnine Amaurosis. (Wien. Med. Woch., Nos. 7 and 8.)—Hildreth. Radical Cure of Entropion and Trichiasis without division of the Skin. (New York Med. Journ., April.)—Joval. On Astigmatism and the Choice of Cylindrical Glasses. (Annales d'Oculistique, Jan.)—Joval. History and Bibliography of Astigmatism. (Ibid., March.)

Fractures.—Rizet. On Shampooing (*Massage*), as a means of Diagnosis of Fracture. (Recueil de Mém. de Méd. Mil., Manch. Methodical friction, performed an hour at atime, by dispersing the tumefaction, often leads to the detection of fracture when there is little or no displacement, while in mere sprains and contusions it is an excellent therapeutical agent.)—Tillaux. Case of Fracture of the Spine, in which Trephining was performed. (Bull. de Thérap., March 15. The case terminated fatally, but the author regards the operation, in certain specified circumstances, as justifiable.)—Hunt. Fracture of the Larynx and Rupture of the Trachea. (Amer. Journ. Med. Science, April. Relates a case in a man forty-five years of age, and refers to twenty-nine recorded cases.)

Galvano-caustics.—Ciniselli. Summary of Investigations on Galvano-caustics. (Gazette Méd., Nos. 12, 13, 14.)

Gun-shot Wounds.—Mitchell. On Gun-shot Wounds giving rise to Remote Paralysis. (New York Med. Journ., March.)—Heine. Gun-shot Injuries of the Lower Extremities. (Langenbeck's Archiv, Band vii, H. 2 and 3. An elaborate description of these injuries as observed by the author in the last Schleswig-Holstein war.)

Hernia.—Lotzbeck. On Para-inguinal Hernia. (Deutsche Klinik, Nos. 12 and 13. Applies this term to a form of inguinal hernia which issues from the canal by an abnormal aperture in the external oblique.)

Hip-joint Disease.—Hueter. Pathological Anatomy of Hip-joint Disease. (Langenbeck's Archiv, B. vii, H. 3.)—Eulenberg. Statistics and Appreciation of Excision of the Hip-joint in Caries.

(Ibid. Founded on fifty-six recorded cases.)—Lefort. Apparatus for the Treatment of Hip-joint Disease. (Gaz. des Hôp., May 5. With woodcut.)—Verneuil. Etiology of Hip-joint Disease. (Ibid.,) June 12.)

Knee-joint.—Verneuil. On Wounds of the Knee. (Bull. de la Soc. de Chir., tome vi, pp. 340—358, 377 and 441. With a discussion.)

Laryngoscope.— Box Apparatus for exhibiting Laryngoscopic Demonstrations. (Deutsche Klinik, No. 15.)—Czermak. On the Removal of Foreign Bodies from the Pharynx by aid of the Laryngoscope. (Jena Zeitschrift. B. ii, H. 4. Four interesting cases given, in which the laryngoscope was the means of rapid detection and removal of the foreign bodies.)—Turck. Instruments for the Removal of New Formations from the Larynx. (Allg. Med. Zeit., Nos. 11 and 16. With woodcuts.)—Cutler. Cases illustrating the Uses of the Laryngoscope and Rhinoscope. (Boston Med. Journ., Feb. 1 and April 5.)—Fieber. On Intra-laryngeal Faradisation. (Wien. Med. Woch., No. 12.)—Prinz. On New Laryngeal Formations, and three Operations for Laryngeal Polypi. (Archiv der Heilkunde, H. 3.)—Voltolini. Removal of a Polypus of the Larynx with the Galvano-caustic. (Langenbeck's Archiv, B. vii, H. 3.)

Orthopædics.—G. H. Meyer. Mechanism of Scoliosis. (Virchow's Archiv, Feb. and May.)—Salzer. On a Peculiar Contraction of the Foot. (Jahrb. d. Gesell. d. Aertze in Wien, Heft 3.)

Ovariotomy.—Péan. Two Successful Cases of Ovariotomy. (Gaz. des Hôp., No. 31.)—Reeves. Successful Case of Ovariotomy. (American Journ. of Med. Science, April.)—Stilling. Cases of Ovariotomy. (Deutsche Klinik, Nos. 1—8. Very detailed accounts of seven cases which have occurred in his practice during 1864-65.)

Polypus.—A. Guérin. Treatment of Naso-pharyngeal Polypus. (Bull. de la Soc. de Chirurgie, tome vi, pp. 518 and 555.)

Scapula.—Michaux. Total Ablation of the Scapula, with Preservation of the Arm. (Gazette Méd., Nos. 16—19.)—Hammer. Extirpation of the entire Scapula, with Preservation of the Arm. (New York Med. Journ., May.)

Stomatoscope.—Klopsch. Description of Brück's Stomatoscope. (Wien. Med. Woch., No, 7. Stated to be highly useful in diagnosis of diseases of the teeth, allowing of the galvanic light being employed for this and other purposes within the cavity of the mouth.)

Testis.—Tillaux. Cystic Disease of the Testis. (Bull. de la Soc. de Chir., tome vi, p. 394.)

Transfusion.—Eulenberg and Landois. Practical Contributions on Transfusion of Blood. (Berlin Klin. Woch., Nos. 9—15. An elaborate experimental investigation, but unaccompanied by any application to the human subject.)

Urinary Organs.—Martini. Report on new Publications on Diseases of the Urinary Organs. (Schmidt's Jahrb., No. 5.)—Mallez. On an Instrument for the Insufflation of Medicinal Powders into the Urethra. (Gaz. des Hôp., April 19. With a woodcut.)—Notta. Two Cases of Lithotomy performed by Nélaton's Pre-rectal Operation. (Union Méd., No. 35.)—Verneuil. On Ectopia of the Prostate and Urinary Fistula. (Archives Générales, June.)—Schilling. On Internal Urethrotomy. (Deutsche Klinik, Nos. 2—9. A good critical estimate of the operation.)—Wyss. On Malignant Disease of the Prostate. (Virchow's Archiv, March. Two new cases, and references to twenty-eight published communications.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond.

Fellow and Examiner in Midwifery, Royal College of Physicians; Obstetric Physician to St. Thomas' Hospital; President of the Obstetrical Society of London.

I. The Non-pregnant State.

On Malformation of the Organs of Generation. Fœtation in a rudimentary Uterine Cornu. By William Turner, M.B.—Mr. Turner relates two very interesting cases of pregnancy in a uterine horn. In one case the left horn of the uterus was rudimentary and pregnant. Rupture took place, with escape of the fœtus into the cavity of the peritoneum. The other case, also one of the uterus bicornis unicollis, had the left horn rudimentary and pregnant. The fœtus was retained after the full period of utero-gestation. In his reflections upon these cases and upon others published, Mr. Turner suggests that some presumed cases of Fallopian pregnancy were in reality cases of pregnancy in a rudimentary horn of a two-horned uterus.—*Edinb. Med. Journ.*, May, 1866.

Rupture of an Inverted Uterus of eight months' standing. By Dr. Emmet.—A young woman was delivered of her second child after a very rapid labour. The after-pains were severe. Hæmorrhage followed. This continued, and was attributed to a polypus until she came under treatment, some months after labour. Reposition was made under ether. The left hand in the vagina, four fingers were passed up as far as possible between the inverted portion and the neck, with the thumb in front, so that the body was encircled. The fingers and the fundus rested in the palm. Then, with an upward and outward pressure at the same time, the neck was gradually dilated until the seat of inversion was reached by the frequent extension of the fingers. This treatment was persevered in, while during the whole time the organ had been lifted above the pubes, so that the other hand could assist in the rolling out of the parts by sliding upwards the abdominal bulbs, with a steady pressure over the posterior portion of the ring formed by the inversion. In less than half an hour the mass, as felt through the abdominal parietes, had doubled in size, the depression in the centre had become larger. The fundus gradually passed entirely within the cervix. The reduction was completed in about an hour and twenty minutes. The patient did well.—*American Journ. of the Med. Sc.*, April, 1866.

On Fibrous Tumours of the Uterus. By R. T. TRACY, M.D.—Dr. Tracy gives the histories of two cases of non-pediculated fibrous tumours of the uterus, giving rise to serious hæmorrhages, which he successfully removed by dilatation of the cervix, incisions into the tumours, and extraction.—*Australian Med. Journ.*, Feb., 1866.

II. PREGNANCY.

Case of Intermittent Fever in a Child in Utero. By Dr. HUBBARD, United States.—Dr. Hubbard related to the Edinburgh Obstetrical Society a remarkable case of ague occurring in the fœtus in utero. During the universal prevalence of maternal fever in Illinois Mrs. C— was delivered of her second child. She had been for many days subject to tertian fever, and had noticed that on the days of the recurrence of the chill there was an entire cessation of the motions of the child. The administration of quinine was purposely withheld, on account of its well-known effects in causing uterine congestion and inducing premature uterine action. When delivered, the child had a well-marked chill, followed by fever. Quinine was then administered to the mother, arresting the disease in her and in the child also.

Sir James Simpson, commenting upon this case, recalled the observations of Morton and Dr. Russell, who had attended to the disease. —*Edinb. Med. Journ.*, June, 1866.

III. LABOUR.

On Dilatation of the Perinæum. By JAMES MORE, M.D.—Dr. More shows that the perinæum dilates, not alone under distending force of the body under expulsion, but by an inherent dilatability.—*Edin. Med. Journ.*, June, 1866.

Use of the Wire-ribbon in some Cases of Difficult Turning. By Dr. HEYERDAHL, of Berlin.—In a case of impacted shoulder Dr. Heyerdahl, finding it impossible to grasp the foot or to slip a tape over it, conceived the idea of forming a loop of wire-ribbon such as is used by milliners for making bonnets. By means of this contrivance he was enabled to seize the foot and effect delivery.—*Edinburgh Med. Journ.*, Jan., 1866.

On Laceration of the Vagina in the course of Labour. By ALFRED

H. M'Clintock, M.D.—Dr. M'Clintock draws a parallel between lacerations of the uterus and lacerations of the vagina. He gives several valuable clinical illustrations of the conditions and symptoms attending the latter lesion. The following are his conclusions:—

1. Premonitory symptoms are very rare.
2. The immediate constitutional effect of a laceration of the vagina is not, on the whole, so profound as that arising from rupture of the uterus.
3. Vomiting is occasionally a symptom of the accident, but it is not of the *coffee-grounds* character.
4. The laceration is very rarely, if ever, induced by deformity of the pelvic brim.
5. The hand is commonly engaged in the pelvis at the time the laceration occurs.
6. The tear can in no way be attributed to contractions of the structure directly involved.
7. The laceration almost takes a circular direction, and remains fistulous, or, at least, shows very little disposition to contract.
8. The escape of the fœtus into the peritoneal cavity follows more frequently upon vaginal laceration than upon uterine rupture.
9. The escape of the placenta, likewise, through the laceration, is more apt to take place here than in ruptures of the uterus.
10. Relapse of the intestine, also, is a less rare complication of vaginal than of uterine laceration.
11. Turning is found to be practicable for a longer period of the laceration of the vagina.
12. There is a greater liability to pelvic abscess of the vaginal laceration.
13. Vaginal laceration is much less fatal than uterine.—*Dubl. Quart. Journ. Med. Sc.*, May, 1866.

Two Successful Cases of Cæsarian Section. By Dr. Baeza.—Case 1. —A woman, aged thirty-eight, had had three difficult labours, the children being all dead. In her fourth pregnancy, at term, she suddenly felt pain, and the fœtus moving outside the uterus. Severe fever followed, and on the third day the placenta was discharged. The fever lasted thirty days, attended by great pain in the abdomen. At the end of this time a large quantity of foul pus flowed from the vagina. A foreign body was now seen projecting below the umbilicus. This swelling burst; the head of a fœtus was seen in the opening. After a few days the patient herself dragged a portion of the tumour out. An incision was made in the abdomen, and the entire skeleton of a fœtus was extracted. The cavity of the cyst was washed out with warm water. The patient recovered.

Case 2.—A woman, aged forty-two, had had seven children. In September, 1863, she was again pregnant. She felt the child at four months, but the pregnancy continued for an unusual time. On the 10th May, 1864, when estimated at term, she felt a severe pain in the right leg which spread to the loins and hips. These increased, and great prostration ensued. At this time the cervix uteri was felt as in the non-pregnant state; a little pure blood, however, issued from the vagina. At the end of July she had renewed pains, and an offensive discharge escaped from the vagina. The pains remitted, and the abdominal tumour diminished. The 10th January, 1865, she came to the hospital. There was then a hard tumour, the size of a seven months' pregnancy. Crepitation was felt in the highest point. The general condition was that of hectic fever. An abdominal section was made on the 15th January. The bistoury struck upon a firm mass, which on section resembled the uterus. The fœtus was found in a cyst, macerated in a quantity of stinking matter, which was washed out by warm water. None of this fluid came out by the cervix uteri. The cyst had several diverticula, one of which seemed to communicate with the uterus. The cyst gradually shrank, and the woman recovered.—*Siglo Medico.*, 1865.

IV. CHILDBED.

Contributions to the Knowledge of the Relation of Temperature in Childbed. By Dr. OSCAR WOLF.—Dr. Oscar Wolf has continued the observations on the temperature of the body in childbed made by Drs. Wunderlich, Winckel, v. Grünewaldt, Hecker, Gierss, v. Bärensprung, and Traube. The observations were much in Marborg and Kiel, and embrace 266 cases. They are restricted to normal childbed. He found the average of forty observations gave 37·39° C. as the temperature immediately after birth; 37·43° C. as the temperature in the morning, and 37·37° C. as that of the evening of the first day following labour.

With regard to the influence of milk-secretion—the so-called milk-fever—upon temperature, he found a marked difference between primipara and multipara. Examined on the third day, milk setting in, the first gave an average of 37·9° C., the second 37·37° C.—*Monatsschr. f. Geburtsk*, April, 1866.

A Case of Embolia of the Pulmonary Artery in a Puerperal Woman. By Dr. F. RITTER.—In this case the patient was pluriparous, æt. 26. The first day of childbed went off quite regularly. Then the lochia became offensive, and slight pain was felt on pressure. Temperature, 38·0° C. These symptoms disappeared eight days afterwards. She suddenly fell back in deep syncope. Consciousness did not altogether leave her. When seen, the face was pale; she seemed dying. Respiration not much accelerated; pulse very small and frequent. She complained of oppression in the chest. Next day these symptoms persisted; the temperature was 36·8° C.; respirations were catching in character, 36; cyanotic lips and tongue. Subsequently, pulse and respirations increased in frequency; temperature fell. She died the third day. *Autopsy*—In the right horn of the uterus was an abscess; inside uterus no disease. In ovarial veins, small, yellow fibrin-clots, apparently formed before death. In the right chief branch of the pulmonary artery was a pale-red delicate thrombus, plugging the vessel.—*Monatsschr. f. Geburtsk.*, Feb., 1866.

Observations on the Temperature in Childbed. By Dr. SCHRODER.—Dr. Schröder has made careful observations on the temperature of women in childbed. They extend to pathological as well as to normal conditions.

In two cases he thought a morbid increase of temperature was due to severe after-pains. Thus, in one case it rose to 38·5° C., in the other to 39·4° C.

More frequently the temperature rose when injuries had been sustained during labour, constituting what v. Grünewaldt called "wound-fever." In thirty-two cases of this kind, seven had perinæal lacerations; twenty-five had slighter rents of the mucous membrane of the vagina. But there is a great difference in the reaction in different individuals. The size was seldom rémarkable, only once over 40° C. It was ushered in by a slight feeling of cold.

The milk-fever is chiefly distinguished from the "wound fever" by setting in later; 39·6° C. was the highest temperature observed. The pulse was seldom over 100. Dr. Schroder then endeavours to distinguish between epidemic and non-epidemic puerperal inflammation. He takes forty-seven cases of para- and peritonitis in which there was circumscribed pain on pressure. These mostly fell ill with light shivering. The temperature rose from 38·5° C, to 41·5° C. In the greater number of cases it was 40·5° to 41°. The highest temperature was generally reached on the first, second, or third day.

Epidemic cases are divided, according to Virchow's scheme, into—1. Inflammatory; 2. Thrombosis; 3. Ichorrhæmic and Septiciæmic In four cases of the first kind, the highest temperature was 41·5° C., with a pulse of 144, and 38 respirations in the minute. In three cases of general diffuse peritonitis a remarkable sinking of temperature towards the fatal termination was observed. In two cases of the third kind in which puerperal sores—a characteristic indication of epidemicity—existed, the temperature was 40·9; pulse, 120; respirations, 22. The observations of morbid conditions are defective.—*Monatsschr. f. Geburtsk.*, February, 1866.

On Puerperal Fœtid Pulmonary Abscess. By Dr. J. A. BYRNE.—This case was that of a primipara who had been ten hours in labour, there having been no complication beyond a little post-partum hæmorrhage. On the third day there was rigor, pain in the pubic and iliac regions. On the sixth day, stitch in the right side, but no cough. On the seventh day violent mania broke out, and lasted several days. When this declined, cough appeared, and a peculiar fœtor was perceived in the mouth. On the twenty-sixth day she expectorated a large bowl-full of a greenish purulent fluid, possessing the most intolerable gangrenous fœtor. Similar symptoms returned from time to time, and the case ended fatally by hectic fever three months after delivery.—*Med. Press*, June, 1866.

On Encysted Serous Pelvic Effusions following Puerperal Inflammation. By Dr. MATTHEWS DUNCAN.—Dr. Duncan describes two very interesting cases of serous collections in the pelvis following labour. The symptoms, general and physical, at first resembled those of ordinary pelvic cellulitis. In one case a tumour formed in the right hypogastric region, extending midway to the umbilicus. The tumour was punctured by vagina, and about nine ounces of fluid, scarcely turbid, and of a light green colour, were drawn off. The tumour then disappeared.—*Edin. Med. Journ.*, April, 1866.

On Puerperal Tetanus. By Dr. W. A. GORDON.—Dr. Gordon relates three cases of a rare but very interesting disease, tetanus, following abortion and labour:—

CASE 1. A healthy temperate Irishwoman, mother of four children, had aborted at an early period, eleven days before being seen by Dr. G. She had got about her household duties on the third day. On the eighth day she sat a long time at the outside door, her feet resting upon the stone doorstep. Next day she complained of pain in the head, which extended to the jaws and throat. Two days later she complained of painful stiffness of the jaws, extending to the back and throat. Intellect unaffected, pulses natural, lochial discharges quite ceased; no symptom of metritis; next day opisthotonos. Death on the fifteenth day after the abortion. Etherisation increased her distress.

CASE 2. May, 1863. A farmer's wife, mother of five children, aged forty. Dr. G. saw her for hæmorrhage, threatening abortion, and signs of metritis. Thirty-six hours later, stiffness of the jaws, neck, and throat, running into severe spasms. Death on the fifth day. Chloroform left her worse. It transpired that she had been subjected to an operation for the procurement of abortion a few days before Dr. G. saw her.

CASE 3. July, 1863. Mother of three children, aged thirty-five. Dr. G. called on account of flooding. Cold applications, ergot, opium, acetate of lead, and tampon of sponge. Abortion was completed, and she seemed to have recovered. Five days after the abortion tetanus set in, and she died eight days later.—*Amer. Jour. of Med. Sci.*, January 1866.

Laceration of the Vagina, with Prolapsus of a Dropsical Ovary. By Professor LUSCHKA.—*Monatsschr. f. Geburtsk.*, April, 1866.

Eclampsia without Kidney-affection; favorable Action of the Subcutaneous Application of Morphine; successful Result for Mother and Child. By Dr. HARTMANN.—*Monatsschr. f. Geburtsk.*, April, 1866.

On the Weight of the Fœtus and its Relations to the different Months of Gestation. By Dr. C. HECKER.

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PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Bidrag til Kundskab om Echinococcernes Udvikling, navnlig om Doettreblæredannelsen.* Af VALD. RASMUSSEN, Prosector i patholog. Anatomi. (Af Naturhist. Foren. Vidensk. Meddelelser, 1865.) 8vo, pp. 29, with two plates.

A Contribution to our Knowledge of the Development of Echinococci, especially with reference to the Formation of Daughter-cells. By VALD. RASMUSSEN, Prosector in Pathological Anatomy. (From the scientific communications of the Natural History Society.)

2. *Bidrag til Kundskaben om Echinococcernes Udvikling hos Mennesket, deres almindelige Pathologi og pathologiske Anatomi.* Afhandling for Doctorgraden i Medicinen af VALD. RASMUSSEN, Prosector ved Kommunehospitalet. *Kjöbenhavn:* H. Hagerup. 1866. 8vo, pp. 78, with two plates.

A Contribution to our Knowledge of the Development of Echinococci in Man, their general Pathology and pathological Anatomy. A Thesis for the degree of Doctor in Medicine. By VALD. RASMUSSEN, Prosector to the Municipal Hospital.

3. *Helminthologiske Undersögelser i Danmark og paa Island med særligt Hensyn til Blæreormlidelserne paa Island.* Af Dr. med. H. KRABBE. Med 7 Kobbertavler. ('Særskilt aftrykt af det Kongl. Danske Videnskabernes Selskabs Skrifter, 5 Række, naturvidensk. og mathem. Afdeling,' 7 Bind.) *Kjöbenhavn:* F. S. Muhle, 1865. 4to, pp. 64.

Helminthological Investigations in Denmark and Iceland, with *special reference to Hydatid Affections in Iceland.* By H. Krabbe, M.D. With 7 copper-plates. (Reprint from the 'Transactions of the Royal Danish Academy of Sciences, 5th Series. Section of Natural and Mathematical Science,' vol. 7.) *Copenhagen:* F. S. Muhle. 1865.

In the first of the above works the author treats the subject of the development and natural history of echinococci from a zoological, in the second from a medical point of view. The works are illustrated with two plates, which, as the author has considerately sent us the copper-plates with the books, we are enabled to reproduce for the benefit of our readers.

Having in our nineteenth volume (pp. 112, *et seq.*), in reviewing the works of Küchenmeister and others, and in our thirty-fifth (pp. 330, *et seq.*), in noticing those of Cobbold, Althaus, Gamgee, &c., made our readers acquainted with the results of the most recent researches on the subject of Helminthology, it will be unnecessary for us on the present occasion to revert to the labours of the writers above referred to, except on those points which bear directly on the investigations of the authors whose works are now before us.

The old controversy, whether echinococci ought to be referred to one or two species, which was again brought forward by Küchenmeister when he divided them into *Ech. scoleciparien*s and *Ech. altriciparien*s, corresponding to the older classes *E. veterinorum* and *E. hominis,* seems to have been at last decided by Naunyn[1] and Krabbe[2] having, about contemporaneously, succeeded in causing scolices of the human echinococcus (*E. altriciparien*s) to be developed in the intestines of the dog to *Tænia echinococcus,* in the same time and in the same mode as was first demonstrated in 1852 by von Siebold with the echinococcus of the ruminants (*E. scoleciparien*s).

Dr. Rasmussen's investigations confirm the view so strongly put forward by Leuckart, that the scolex-formation always proceeds in brood-capsules developed from the parenchymatous layer of the mother-cell, and being in continual connection with the same.

> "As to the development of the brood-capsules themselves, the results of my investigations agree, with the exception of some less essential points, with those of Naunyn. In the knob-shaped first germ of the brood-capsule, which consists of small nuclear bodies, there is not formed, from the first, a cavity circumscribed by a cuticle, as is generally supposed, but this is preceded by a differentiation, these nuclear bodies being changed in the interior of the

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[1] 'Archiv f. Anatomie und Physiologie,' du Bois-Reymond und Reichert, 1863.

[2] 'Ugeskrift for Læger,' 1864.

knob into a more finely granular mass, and the cuticle in this stage (Plate I, fig. 1) is perhaps only the optical expression of this differentiation. But when the cavity is gradually developed, and especially when the scolex-germ begins to grow inwards, the contour becomes sharper, and assumes the character of cuticle (Plate I, fig. 2), and bears on its inside a fine, granular, parenchymatous layer, which, on account of the parenchyma lying externally to it, visible through the thin cuticle, is difficult to observe, though it is nevertheless present, both in this stage and in the fully developed brood-capsule. This parenchymatous layer supports the vessels entering through the stalk of the brood-capsule, which spread over its inner surface, to enter into each individual scolex in the well known manner. On the bursting of the brood-capsules, it is this parenchymatous layer which partly holds the scolices together, and which is also mentioned by earlier writers, as by Busk,[1] who states that the heads of the echinococci are attached to a common central, granular mass, with short stems, but especially by Huxley,[2] whose drawings (Plate XXIX, figs. 8 and 9) give the best and most true to nature representations we possess of this vascular parenchymatous layer, as well as of the whole brood-capsule; but his statement of its development from the outside of the parenchymatous layer of the mother cell, and his view of the everted, not yet fully developed scolex-germs as blighted scolices, are, of course, incorrect. It is such ruptured brood-capsules, to whose parenchyma the scolices are everywhere attached, which are represented by Huxley (Plate XXVIII, fig. 4), and are by him interpreted in a very complicated manner as secondary cysts,[3] with internal echinococci, such as are developed in these secondary cysts, and external, which are freely developed from the inside of the parenchyma of the mother-cell, and only by their accidental position in the place, where the secondary vesicle is developed, come to stand in relation to the latter. While this parenchymatous layer on the inside of the brood-capsule has so strongly attracted Huxley's attention, that he almost overlooks the cuticle, it is remarkable that so distinguished an investigator as Leuckart does not mention it at all, especially as it is of the greatest importance in the formation of daughter-cells." (P. 5.)

The daughter-cells are formed either endogenously or exogenously with respect to the mother-cell, but these two forms are not strictly separated, and we can therefore not found any species—as Kuhn wished to do—or even variety thereupon, for they may often be developed together: in cells which are developed endogenously, new cells may be again produced both endogenously and exogenously.

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[1] 'Transactions of the Microscopical Society,' 1844, vol. ii, p. 15, London, 1849.

[2] 'Proceedings of the Zoological Society of London,' 1852, p. 116.

[3] These secondary cysts, corresponding to our brood-capsules, he supposes to be formed from the outside of the parenchymatous layer, as inversions from the latter, which are finally separated and set free.

The endogenous cell-formation proceeds exclusively from the brood-capsules.

> "We meet in cells of the size of a walnut or more, and also, though exceptionally, in those of the size of a filbert, in addition to the ordinary brood-capsules, with others still adherent to the parenchyma of the mother-cell, or floating freely in its fluid, which are distinguished by a striking granular parenchymatous layer on their inside. The cuticle then soon begins to increase in thickness and to become laminated, whereby the vesicle acquires more rigidity, and assumes a globular form, while it at the same time increases in size, and the outer parenchymatous cells disappear (Plate I, fig. 4). The scolices may still be preserved, but numerous fat-granules are soon deposited in their parenchyma, and the suckers are destroyed; the scolices now gradually disappear, the anterior part with the circlet of hooks remaining longest, then only this, with some parenchymatous débris and calcareous bodies remains,[1] and at last only some hooks and calcareous bodies are to be found scattered over the parenchyma; finally, these also disappear, and we have a little cyst, perfectly resembling the first acephalocystic stage of the echinococcus cell, developed by the metamorphosis of the tænia embryo; this grows now in the same manner as the former, by the deposition of concentric cuticular layers internally from the parenchyma. I never succeeded in finding with perfect certainty vessels in such cysts, at most I saw only fragments of such vessels (Plate II, fig. 1 *g*), notwithstanding that they were undoubtedly present here, as well as in the brood-capsules, and it is precisely through these vessels that resorption of the scolices takes place." (P. 11.)

Sometimes divided cysts are met with (Pl. I, fig. 7) where only one part is developed into a daughter-cell, while the other retains its original appearance. Naunyn gives a drawing (*l. c.* Pl. XVI, fig. 9), which shows that he has seen the same.

> "Such subdivisions of brood-capsules, with subsequent metamorphosis to daughter-cells, seem to be capable of several repetitions in one and the same cyst. A preparation represented in Plate II, fig. 5, may serve to illustrate this. The whole was found swimming freely in a secondary cyst about as large as an egg. This preparation consists of a larger cyst, (*a*) with laminated cuticle, and, including many decaying scolices, consequently it is a tertiary cyst, developed from a brood-capsule. In different parts of this larger cyst four smaller cysts (*b, c, d, e*) are seen, with thick, laminated cuticles, whose outer layers intersect with that of the larger cyst, with the exception of one (*d*); all enclose the numerous, up to about ten, partly well preserved, partly decaying scolices." (P. 13.)
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> "Beside the daughter-cells, developed in the manner described, we find some in which the cuticle is strikingly thin in proportion to

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[1] It is such a stage which Cobbold represents, after Busk's drawing, without any other designation than degenerated daughter-cells, Plate XIV, fig. 3.

the thick parenchymatous layer, and sometimes it is ruptured, while the round form is often completely preserved. We then see (Plate II, fig. 4) a greater or lesser number of scolices adherent to one another and attached to the copious granular parenchyma, some are still well preserved, others are destroyed in their contours by commencing fatty degeneration, and of others again we see only the circlet of hooks remaining; in short, we have all the same changes which occur in the brood-capsule in its metamorphosis into daughter-cells, only the cuticle is wanting." (P. 14.)

Brood-capsules sometimes occur containing only one scolex. These capsules are developed in the same manner as those containing many scolices:

"The parenchyma on the inside of the brood-capsule increases in thickness, the capsule itself grows, and the scolex may still, when the capsule has attained a very considerable size, and acquired a laminated cuticle, continue unchanged, or only have fat-granules deposited in its parenchyma (Plate I, fig. 5); but at length the resorption commences in a centripetal direction, and the contour is effaced; finally, the circlet of hooks and the calcareous bodies disappear, while during all this the capsule steadily increases in size; at last we see only some calcareous bodies and hooks scattered through the parenchyma, and we have again the first acephalocystic stage (Plate I, fig. 8). We can then scarcely distinguish such capsules as have contained only a single scolex from others which have contained several, for the number of the hooks is only an uncertain distinctive mark." (P. 17.)

But the course of this development presents many shades of variety which, before the eye is properly educated, may easily mislead. Plate II, fig. 3, represents a circlet of hooks still coherent or scattered, which has evidently belonged to a single scolex.

"Now that the scolex during the whole of this development plays only a passive part, is strikingly proved, in addition to the arguments already brought forward, by a preparation (Plate I, fig. 6) which was likewise found in a secondary cyst. This represents a well-developed echinococcus-cyst with laminated cuticular and parenchymatous layers, and in the midst an ex-capsuled scolex, with cuticular layer, which therefore cannot have taken any part in the development of the cyst, while its presence is nevertheless an indubitable sign, that this daughter-cell has been developed from a brood-capsule containing a single scolex. Similar encystings are not so very rare within daughter-cells, which still contain scolices. Thus, this is seen in Plate II, fig. 2, which strongly resembles Naunyn's fig. 4; he has, unfortunately, no drawings of free scolices in different stages of their development to daughter-cells. We see a comparatively thick cuticle, and within it remains of a circlet of hooks; the anastomosing net, mentioned by Naunyn, which, however, from his own description, seems to be only fat, was not met with.

"I do not, however, believe that these cuticular formations stand in any relation to incipient daughter-cells, and I should be more inclined to consider them as accidental, or, it may be, pathological developments of cuticle from the parenchyma; still, further investigations on this point are desirable. Sometimes two scolices are seen thus encapsuled (Plate II, fig. 4, *d*)." (P. 18.)

"*The Exogenous Cell-formation.*—In daughter-cells which have been developed, in the mode described, from brood-capsules, and in which recognisable remains of scolices may still be found, and especially in those which are comparatively large and thin-walled, we see hernia-like eversions from the parenchymatous layer pushing the cuticle before them. The opening of the eversion is sometimes rather striking, in other cases it is very fine and difficult to recognise in profile. The eversion, in which therefore both the parenchyma of the mother-cell and the cuticle take part, continues to increase by independent growth, the cervix becomes more and more constricted, and at length we have a little cyst, formed of a cuticular layer with an internal parenchyma, and attached by a slender stalk to the mother-cell; finally, this separates completely, becomes free, and is now developed like any other echinococcus cyst in the acephalocystic stage. I have not observed any scolex-formation in such daughter-cells, nor in those which are developed from brood-capsules; if every trace of scolices has disappeared in the brood-capsules, these two forms, so different in their development, are no longer distinguishable from one another.

"These eversions may be repeated several times on a single little cyst, and the cyst represented (Plate II, fig. 1) exhibits this daughter-cell formation in three different stages. It was found free within a secondary cyst, is consequently itself a tertiary cyst, and the cysts exogenously developed from it, of which one (*d*) is about to separate, are therefore quaternary. It is evidently the same development which has occurred in primary cysts to Kuhn, Davaine, Leuckart, and indeed also to Naunyn, but which was by them interpreted in a different manner. It is not difficult to understand how the types may have appeared to Leuckart, which did not seem to him to admit of any other explanation than that the first germ of the daughter-cell formation took place between the cuticular layers of the mother-cell; for, to continue to use the simile already employed, this hernia-like eversion will at a certain stage come to lie in its hernial canal, the wall of the mother-cell, whose thick cuticular layer, when the condition is not very favorable, will easily be able to cover the fine canal (Plate II, fig. 1, *f*). The condition is therefore incomparably more favorable for observation in these thin daughter-cells, so that in this case it is not easy to be deceived. The bulgings recently described and represented by Friedreich[1] in the so-called multilocular echinococcus-tumour (fig. 5) seem to be only foldings of layer-cells in narrower spaces (in this case the biliary ducts), such as often occur in the echinococci of the ruminants, and not to be the commencement of exogenous cell-formations, as he assumes; they have at all events nothing in common with the development here described." (Pp. 20—22.)

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[1] 'Virchow's Archiv,' 1865, Band 33, Heft 1.

"But it is not common to meet with this exogenous formation of daughter-cells, and it is certainly only a comparatively small proportion of the numerous cells contained in a larger cyst which are developed in this mode. The great bulk are formed endogenously from brood-capsules. But it appears, from these investigations, that where the exogenous cell-formation occurs, it begins at an earlier stage, and precedes the development of scolices, and therefore also the endogenous formation of daughter-cells." (P. 22.)

As to the signification of this daughter-cell formation in general, it may be assumed that it does not indicate a progressive development of generations, for scolices in the daughter-cells do not exhibit any other than individual differences from those which are formed in the mother-cells; all scolices in a compound echinococcus cyst are equally adapted for development, under certain conditions, to the state of *Tænia echinococcus.*

The second, or more pathological, of the above-named works, opens with a comprehensive and interesting historical sketch of its subject, from the time of Hippocrates and Galen, who only indistinctly mention Hydatids, more accurately spoken of by Aretæus, to the present day. It was not, however, until the close of the seventeenth century that the fact began to be recognised, that some of what were designated hydatids were living animals. This was first suspected by Redi,[1] of the *Cysticercus pisiformis.* The same thing was observed, in the following year by Hartmann,[2] of the *Cysticercus tenuicollis* in the mesentery of the goat. Independently of these two, Tyson,[3] in 1691, describes both cysticerci and echinococci-cysts; in the first, he distinctly observed the white, more opaque neck, turning out on the application of warmth. Malpighi[4] describes the head in the Cysticercus.

But the facts observed by these men appear to have been almost forgotten, until Pallas again brought them forward. He[5] discovered the similarity between the cysticercus and tænia with respect to the crown of hooks and suckers, and he therefore called the first *Tænia hydatigena.* Echinococci occurring in the liver of the sheep and ox were observed by him, and suspected to be living. These he designated *Hydatides singulares*, and his suspicion respecting them was subsequently fully confirmed by Goeze,[1] the celebrated priest of St. Blasius' Church, in Quedlingburg. Goeze, before his death, had the opportunity of examining hydatids from the human subject, his description and drawings of which were afterwards published by Zeder.[2] The material submitted to investigation consisted of a compound echinococcus-cyst with daughter-cells, varying from the size of a nut to that of an egg. Zeder adds an observation of his own, of hydatids in the ventricles and at the base of the brain in a young girl, of which one cyst, of the size of an egg, contained two smaller ones. They were unprovided with capsules. His hydatids were evidently echinococci without capsules, as they are most frequently met with in the natural serous cavities, and this is the more probably the case, as we are as yet unacquainted with any reliable example of the occurrence of cœnurus in man.

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[1] 'Osservazioni intorno agli animali viventi, che si trovono negli animali viventi.' Firenze, 1684.

[2] 'Miscel. curiosa s. Ephemerid. med. phys. german. Academ. Decur. II, anno 1685;' Norimbergæ, 1686, p. 152.

[3] 'Philosophical Transactions,' No. 193; quoted partly in the original words in the 'Cyclopædia of Practical Medicine,' London, 1833, art. "Hydatid."

[4] 'Opera posthum.,' Amstelod. 1698, p. 84, pl. x; drawing not perceptibly magnified.

[5] 'Miscellan. zoolog.,' p. 173, 1766.

Laennec,[3] in a paper read by him in 1804, before the *Société de Médecine*, divided Zeder's genus Polycephalus into three species—*P. humanus*, *P. cerebralis*, *P. granulosus*. But he seems to have given himself entirely to the examination of the walls of the cysts, without having devoted any more accurate and particularly microscopical investigation to their contents. In fact, the great progress towards a correct appreciation of the nature of hydatids made by Pallas and Goeze in recognising the resemblance between certain hydatids and the heads of tapeworms, and in showing that entozoa, like other animals, were developed from ova of their like, was soon forgotten, to be more than regained in a period bordering upon our own day. Rudolphi,[4] the most eminent helminthologist of his time, divided hydatids into three classes—*Cysticercus*, *Cœnurus*, and *Echinococcus*; subsequently, in his 'Entozoarum Synopsis' (1819), he added a new genus, *Anthocephalus*. This last was, however, soon given up. Dr. Rasmussen confines himself to the genus *Echinococcus*. It comprised the cysts in which a greater or less number of worms were included in a common cyst, and was divided into three species—*E. hominis, E. veterinorum*, and *E. simiæ*. The *scolices* (echinococci heads) he saw less distinctly than Goeze and even Zeder had done.

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[1] 'Versuch einer Naturgeschichte der Eingeweidewürmer,' p. 258, Blankenb. 1782.

[2] 'Erster Nachtrag z. Goeze's Naturgeschichte,' Leipzig, 1800, p. 310.

[3] 'Mémoires de la Faculté de Médecine de Paris,' p. 81, 1812; quoted from Livois, "Recherches sur les Echinocoques chez l'homme et chez les animaux, Dissert.,' Paris, 1843, where copious extracts from the original are given, pp. 23, *et seq.*

[4] 'Entozoarum sive Vermium Intestin. Historia Naturalis,' Amstelodami, 1810, vol. ii, part 2, p. 215.

Rudolphi's contemporary, the equally distinguished Bremser, first found echinococci heads in man in an hydatid sac, extirpated from the clavicular region in a woman, and sent to him by Professor Kern; the tumour was about the size of a hen's egg, and contained about thirty cysts. Echinococci had thus been met with in man so far by three observers—Goeze, Rudolphi, and Bremser. In the following year their existence in hydatids from the ventricles of the human brain was confirmed by Rendtorff.[1]

In 1843 the almost constant presence of echinococci in acephalocysts was demonstrated by Livois.[2]—

> "It was 'the genial Danish natural historian,' Steenstrup, whose celebrated theory of conversion led subsequent investigators upon the right track. Although he himself did not speak decidedly of the relation of hydatids, especially to tapeworms, he says that there is much to show that they are only nursing generations, and that the same fate awaits them as has befallen v. Siebold's 'sexless Trematodes,' namely, that they, like earlier forms of development or generations of other animals, must disappear from the system.[3] One may with truth, and we do so with pride, subscribe Leuckart's words on this occasion—'That we must admire the great man's acuteness, which leads him by anticipation to recognise the truth, even where he is not in a position to prove it.'" (Pp. 14, 15.)

A few years later the suspicion that hydatids were only imperfectly developed animals, and especially earlier stages of tapeworms, was decidedly expressed by Dujardin.[4] About the same time von Siebold very decidedly announced the same opinion,[5] though in points of detail he differed from Dujardin—

> "The happy thought then occurred to Küchenmeister,[6] which Goeze[7] had already suggested, to institute experiments by feeding. The perfect success which crowned these experiments has not only brought his name from the little Saxon town of Zittau, to be known

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[1] 'Dissertatio de Hydatidibus præsertim in Cerebro Humano repertis,' Berolini, 1822.

[2] 'Recherches sur les échinocoques chez l'homme et chez les animaux,' Paris, 1843.

[3] 'Om Forplantning og Udvikling gjennem vexlende Generationsrækker. Kbh.,' 1842, s. 60, "On Propagation and Development through changing series of Generation," Copenhagen, 1842, p. 60.

[4] 'Histoire naturelle des Helminthes, 1845.

[5] Wagner's 'Handwörterbuch der Physiologie,' 2 Bd., pp. 676, *et seq.*, 1844.

[6] 'Ueber die Umwandlung der Finnen in Tænien,' "Prag. Vierteljahrschrift,' 1852, 1 Bd., p. 106.

[7] Loc. cit., p. 292.

throughout the whole world, but has at the same time completely cleared up this question, which seemed likely to be lost in barren, though ingenious, hypotheses. Küchenmeister succeeded, in fact, in changing the *Cysticercus pisiformis* of the rabbit into a sexually mature tapeworm (*Tænia serrata*) in the intestine of the dog. The correctness of this experiment was soon corroborated by v. Siebold,[1] who added a new one, as by feeding with cysts of the *echinococcus veterinorum* he obtained a new little tapeworm, the three-jointed tænia echinococcus, developed in the intestine of the dog." (Pp. 15, 16.)

Küchenmeister succeeded, moreover, in performing the converse of the above experiments, producing hydatids from mature tapeworms; thus, from the *Tænia serrata* he obtained the *Cysticercus pisiformis*, from the *Tænia cænurus* the *Cænurus cerebralis*, from the *Tænia solium* the *Cysticercus cellulosæ*, &c.; and these results were soon confirmed by Leuckart, Eschricht, v. Beneden, &c.—

> "The different stages in the development of hydatid tapeworms are, therefore, five—1. The six-hooked embryo. 2. The hydatid (*cysticercus*). 3. Head of the tapeworm (*scolex*). 4. The jointed tapeworm (*strobila*). 5. The sexually mature, isolated joint of the latter (*proglottis*). The last-named was looked upon by Steenstrup as an independent individual, which, by throwing off knots, is developed from a nurse (*scolex*), for which, again, the six-hooked embryo is grand-nurse. Hence, there are three generations—1. The six-hooked embryo, 2. Scolex, 3. Proglottis. With respect to the echinococci, matters are more complicated." (P. 16.)

Having thus, at the risk of being considered prolix, brought before our readers the principal epochs in the history of the investigations carried on from time to time respecting the nature of hydatids—in doing which we have, of necessity, omitted many interesting details—we shall conclude this article by briefly referring to some points connected with the pathology of the subject. We may, however, first remark in passing that the author, in his chapter on the history of the development of echinococci, refers at some length to the labours of Dr. Krabbe, already noticed in our 35th volume, p. 139.

As to the *different forms* which occur in the human subject, the echinococcus in man is, as a rule, *altricipariens*; nevertheless, there are cases where it continues to be *scolecipariens*. Observations of such single echinococci are very rare, particularly if we exclude the cases where numerous multiple and single echinococci present themselves beneath the peritoneal coat, together with large compound echinococci in the liver.

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[1] 'Zeitschrift f. wissenchaft. Zoologie,' 1853.

The single echinococci cysts occur especially in the lungs, where they are the most frequent form; but they are met with also in the larger parenchymatous organs. In four cases of echinococcus observed by the author during the last two years, the cysts were in two single, and contained numerous scolices. Both these occurred in Icelanders; the cases are reported.

The number of daughter-cells in the compound echinococci varies considerably, as in a case observed by Krabbe,[1] where a cyst, of the size of a child's head, in the lung, contained only two daughter-cells, about as large as nuts, with scolices, and in a quite similar one of Andral's.[2] Cruveilhier[3] states that he saw cysts with two, three, and four daughter-cells. But the number, if the older accounts are not exaggerated, may reach several thousands. In a case quoted by Plouquet, Davaine mentions 9000, and Leuckart adduces an instance where an hepatic echinococcus, weighing thirty pounds, in a woman aged 60, contained at least some thousand daughter-cells.

The simultaneous occurrence of both the above forms of echinococcus in man is very rare; the multilocular echinococcus is met with only in man, and not outside the liver.

As to their existence in *different organs*, they may be found in almost all organs and tissues, but with very different frequency. The liver is their favorite abode, and they are there met with more frequently than in all other organs together.

The *number of the cysts* is in general very limited, most frequently there is only one, and usually in the liver; or there are only a few, and then either in the same organ or in an adjoining one. But, as exceptions to this general rule, there are perfectly reliable observations, where the number of the cysts increases very considerably, even up to 1000 (Budd).[4]

Etiology.—The older statements respecting the cause of the hydatid disease have no longer any weight, especially with regard to that most generally assigned, external violence. We now know that the disease in question proceeds exclusively from ova of the tænia echinococcus introduced into the intestinal canal. But this is quite consistent with some former observations, such as the remark that the disease is very rare among seamen and those who live on the coast (Schleisner); and that of Budd, that the rich are less frequently attacked than the poor, and especially than those who inhabit damp localities; but not, as he states, because the latter live almost exclusively on vegetable food, but because in such parts, from the occupation of the people, dogs have more opportunity, at the slaughtering of cattle, of swallowing the echinococci cysts. It is also natural that the rural population should be more frequently attacked than that of the large towns. But in what way the dog is enabled to transfer the ova of the tapeworms to man—whether through the food, water, or in some other medium—it is difficult to decide; but, at all events, the transference seems to be highly promoted by uncleanliness (Iceland).

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[1] 'Hospitals Meddelelser,' 2den Række, 1ste Bind, p. 142.

[2] 'Clinique médicale,' tome 2, 2ème édit., Paris, 1829, p. 407.

[3] 'Traité d'anatom. pathol. générale,' t. ii, p. 97, Paris, 1852.

[4] 'On Diseases of the Liver,' 2nd edition, London, 1852, p. 431.

Age and Sex.—According to Schleisner the disease occurs at all ages, but most frequently in men between thirty and forty, in women between forty and fifty; it is very rare in children.

As to the *geographical distribution* of the disease, we have already, in our notice of Dr. Krabbe's work, in our 35th volume, above alluded to, mentioned its excessive prevalence in Iceland, where dogs abound.

Duration, General Effects and Results of the Echinococcus Disease.—The echinococci are of very slow growth, the average duration being from five to ten years. Dr. Budd records a case where the disease lasted sixty-eight years.[1] In other instances, however, their growth seems to be more rapid; thus, Velpeau[2] removed a tumour of the size of a nut, which had developed itself in the course of six months, from the axilla of a girl aged twenty-two—

> "This growth may be arrested and the parasite may perish, either by absorption of the fluid and collapse of the individual cysts, or by suppuration and subsequent atheromatous metamorphosis of pus. But, except in the cases where such spontaneous death occurs, the tumour grows; and although in the commencement, provided it be not situated in the eye or the brain, it produces no inconvenience, especially no pain, or at all events only a slight feeling of weight and tension, and may attain even a considerable size without essentially disturbing the function of the organ concerned; nevertheless, the individual's health is gradually undermined, when the tumour has exceeded a certain magnitude, varying according to the importance of the organ in which it has its seat. If this be one of the parenchymatous organs (the liver, lung, spleen, kidney), the gradual displacement of the parenchyma will, of course, even if no rupture or suppuration in the sac or its surroundings take place, slowly act on the general health, the patient will emaciate and sink under increasing hectic; but often an intercurrent disease, especially pneumonia, becomes the proximate cause of death." (P. 73.)

In the external connective tissue the tumours acquire only slight importance, and they occur here almost always without surrounding inflammation. In the connective tissue in the cavities the results may, of course, be more serious, in consequence of pressure on canals, mechanical obstruction to parturition, &c. In the bones the parasite is developed in the medullary cavity, with or without deep and fixed pain; swelling of the bone, atrophy and fracture may occur.

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[1] Loc. cit. p. 422.

[2] 'Gazette des Hôpitaux,' 1857, p. 396.

Echinococci in the eye and brain are the most dangerous. In the eye they have been seen only once by Gescheidt, in contrast to the cysticercus, which has been met with and diagnosed by means of the ophthalmoscope thirty or forty times by von Graefe alone. In the brain the symptoms vary accordingly as the parasites are developed in the arachnoid, the ventricles or the cerebral substance itself.

Though life may often be long preserved where even large hydatids exist, the latter being frequently discovered only accidentally on dissection, rupture, with all its dangerous chances, remains in the background. The result of this will of course vary with the direction in which the bursting may take place, and whether the contents of the tumour escape outwardly or are discharged into, for example, the peritoneum or the vascular system. Piorry[1] relates a case in which a large hepatic cyst discharged its purulent contents with remains of hydatid membranes into the perforated vena cava, filling with the mass the right side of the heart, the pulmonary artery and its branches. The patient died in the course of three hours. Lhonneur saw a cyst open into the inferior cava; Luschka and Leuckart found branches of the pulmonary artery plugged with echinococci vesicles from a large hepatic cyst, which had opened into the superior cava.

For the convenience of our readers we shall append to this notice of Dr. Rasmussen's interesting work, which exhibits the results of so much careful research and diligent personal observation, a translation of the explanation of the plates with which his books are illustrated.

Since the foregoing was written, we have received from Copenhagen a handsome quarto volume, illustrated with seven very beautifully executed copper-plates, from the pen of Dr. Krabbe, of whose eminent helminthological labours we have already spoken, both in the present article and in our 35th volume. The object of Dr. Krabbe's work is to arrive at certainty with respect to the source in Iceland of the so-called hydatid disease endemic in that country, with a view to discover how far and by what means its occurrence may be most advantageously counteracted. Some of his results will be found in our former notice, vol. xxxv, p. 139. Two of the three sections of the present volume are taken up with minute details respecting the intestinal worms found in dogs and cats in Copenhagen and in Iceland, into which we cannot here enter; we shall therefore pass to the third chapter, on the hydatid affections of Iceland, and shall endeavour to give a brief abstract of the same.

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[1] 'Percussion médiate,' 2de éd., p. 169.

It appears that a regular system of medicine has existed in Iceland only during the last 100 years; our knowledge of the echinococcus disease in that country is therefore not of ancient date, though there is reason to believe that this affection has been epidemic in the island for centuries, as it is at present. The first authorised physician in the island, Bjarne Povelsen, one of the authors of the excellent description of the country published in 1772, was appointed in 1760. Povelsen, John Petersen, author of an Icelandic medical work, 1775-1801, and John Svendsen, 1794, in their descriptions of the "malum hypochondriacum," of the "Hepatalgia omnis generis maxime frequens," &c., all show that cases of the hydatid disease, though not fully recognised, had come under their observation.

In 1803 there were in Iceland only five district surgeons, and two unexamined, who had permission to practise; and the number of medical men has since continued much the same. In their reports the disease is constantly mentioned from all districts of the country as general, and as one of the most important chronic diseases; but it has usually been looked upon as a disease of the liver, and called *Hepatalgia, Hepatitis chronica, Infarctus* or *Hypertrophia hepatis,* while it is only occasionally that the name hydatids has been applied to it.

Outside Iceland it can scarcely be said that any general occurrence of the disease has been specially observed in the first decennial periods of this century.

As usual, the echinococci-cysts occur most frequently in the abdominal organs, particularly in the liver, whence they are not unusually evacuated into internal hollow organs, or by suppuration through the abdominal wall, or, after having perforated the diaphragm, through the lungs or intercostal spaces. The efforts of the Icelandic practitioners are, of course, directed principally to evacuate them through the skin, and the most usual method has been puncture with a trocar. Finsen has treated many successfully by cauterization after Recamier's method. A remarkable phenomenon which the author has ordinarily observed is, that the evacuation of echinococci-cysts into the peritoneal cavity is accompanied, as it would seem rather constantly, with a transitory nettle-rash.

Examples are also recorded of the occurrence of echinococci superficially beneath the skin; and it would appear that in this way they are most frequently developed around the upper part of the thorax. Such tumours have usually been removed by operation.

Echinococci-cysts act essentially as foreign bodies, and by their pressure produce various functional disturbances. They may or may not in themselves give rise to general symptoms. By bursting, which does not unfrequently happen as the result of contusions, as well as when suppuration occurs about them, they may easily cause dangerous symptoms; but such cases may end also in spontaneous recovery.

The author details some experiments instituted by himself and others with a view to determine from what tapeworm the echinococci of the Icelanders derive their origin, and whether the latter are different from those which occur in the domestic animals. Our space will not permit us to enter on a review of these experiments; and this is the less necessary, as we have already, in noticing Dr. Krabbe's former work in our 35th volume (p. 139), mentioned his principal results, as well as his suggestions for preventing the spread of the disease, including the imposition of a dog-tax. In conclusion, we have only to express our admiration of the exhaustive manner in which the author has treated his subject, and of the careful and highly creditable style in which the volume at present under our notice has been brought out.

PLATE I.

Fig. 1. Incipient formation of brood-capsules in a secondary cyst; *a*, the knob-shaped pivot; *b*, commencing formation of a cavity in the same by differentiation of the parenchyma; *c*, wall of the mother cyst.

Fig. 2. Later stage of the brood-capsule: *a*, scolex-knob; *b*, cavity with cuticle and internal parenchyma; *c*, external parenchyma; *d*, wall of the mother cyst.

Fig. 3. Brood-capsule with a single scolex: *a*, its outer cellular layer; *b*, its cuticle; *c*, internal parenchyma; *d*, scolex.

Fig. 4. Daughter-cyst formed of a brood-capsule: *a*, cuticle; *b*, internal parenchyma; *c*, scolices.

Fig. 5. Daughter-cell, developed from a brood-capsule with a single scolex: *a*, cuticle; *b*, parenchyma; *c*, scolex.

Fig. 6. A similar cell with capsuled scolex (*a*).

Fig. 7.[1] Cut-off brood-capsule, of which only the one part is developed to form a daughter-cell: *a*, cuticle of the daughter-cell; *b*, parenchyma; *c*, cuticle of the brood-capsule; *d*, scolex.

Fig. 8. Daughter-cell, developed from a brood-capsule (magnified about 60 times): *a*, cuticle; *b*, parenchyma; *c*, calcareous bodies; *d*, hooks.

Fig. 9. A similar constricted daughter-cell (magnified 30 times): *a*, cuticle; *b*, parenchyma; *c*, a hook; *d*, calcareous bodies.

PLATE II.

Fig. 1. Tertiary daughter-cell, with exogenous development of quaternary cysts (magnified 220 times): *a*, cuticle; *b*, parenchyma; *c*, persistent circlet of hooks; *d*, constricted petiolate cyst; *e*, eversion; *f*, same commencing.

Fig. 2. Daughter-cell with perishing scolices and internal cyst-formation (ex-capsuled scolex): *a*, cuticle; *b*, parenchyma; *c*, circlet of hooks; *d*, internal cyst; *e*, its cuticle.

Fig. 3. Daughter-cell, developed from a brood-capsule with a single scolex: *a*, cuticle; *b*, parenchyma; *c*, perishing scolex.

Fig. 4. Parenchyma (entocyst) of a burst daughter-cell with perishing scolices: *a*, parenchyma; *b*, scolex; *c*, crown of hooks of the same; *d*, two capsuled scolices.

Fig. 5. Class-formed daughter cyst, developed from a brood-capsule by constrictions: *a*, principal cyst; *b*, *c*, *d*, *e*, cut-off cysts.

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[1] This figure is, in contrast to the others, which are accurately drawn from the preparations, copied from a sketch, but is in all points essentially correct.

Review II.

A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits; illustrated by numerous Cases and Engravings. By W. Roberts, M.D., Physician to the Manchester Infirmary, and Lecturer on Medicine in the Manchester School of Medicine. *London,* 1865, pp. 523.

Regarded from the physician's point of view, this book may take its stand as one of the best works extant on urinary and renal diseases. It is evidently the work of a physician, and will be welcomed all the more heartily by the medical public from the fact that it is essentially clinical, and is shorn of all the mass of *chemical* minutiæ which it has lately been the work of specialists to evolve and amass. The author himself makes allusion to the "vast array of researches on the composition of the urine and rate of excretion of its several ingredients accumulated in recent times," but he wisely declines to attempt even an abstract of them "until they can be shown to possess some clinical value." The credit justly due to him for the intrinsic merits of the book is much increased by the circumstances under which it has been written. Dr. Roberts is a provincial physician, and while on the one hand the field for observation of medical (more particularly acute) disease must be more limited in a provincial than in a metropolitan hospital, on the other hand the literary difficulties inseparable, even in London, from the composition of any large work, must weigh very heavily on an author in a provincial town. The book, however, bears evidence of being thoroughly up to the standard of the present day. The cases, some of which are given in full to illustrate descriptions of disease, are partly from his own practice, partly gleaned from the works of different writers, and from various journals and periodicals. Each chapter is headed by a list of references to the best authorities on the subject of which the chapter treats. This plan, which is now largely in vogue in Germany, might well be more frequently adopted in this country, in that it saves the reader the great inconvenience and discomfort of referring to foot-notes. The engravings are indifferent—some of those which illustrate the calculi rough and unfinished.

The volume is divided into three parts.

Part I treats briefly and concisely of the physical and chemical properties of the urine both when normal, and, as far as they have a practical value, when deviating from the normal standard. His experiments confirm fully the discovery of Bence Jones (denied by Vogel and others), that the urine is always less acid, often alkaline, after a meal; but he explains the phenomenon differently. Bence Jones attributes this temporary alkalinity of the urine to a monopoly of acid by the stomach, which pours out abundantly its acid gastric juice at the time of digestion. Roberts asks how, if this be so, is explained the fact, that the alkalinity of the urine does not appear for some time (an hour or two) after a meal, whereas the flow of gastric juice into the stomach commences as soon as the food is swallowed.[1] He believes that it is the alkaline bases, contained so abundantly in the food and supplied to the blood in excess when digestion is over, which are then secreted in excess by the kidneys, and, for a time, render the urine alkaline. This view no doubt appears strengthened by the fact, that the more of the alkaline bases the food contains, the more marked is the change in the reaction of the urine; but it is adverse to the fact that a similar decrease of alkalinity of the urine is producible by the action of emetics.

The most powerful *acidifiers of the urine* are carbonic and benzoic acid; but urine that is habitually alkaline cannot be acidified by the exhibition of acids.

The question of *oxaluria* is very well and fully discussed, and objection is taken to the "oxalic acid diathesis." There is no direct connection between a certain train of symptoms, of which the principal are languor, dyspepsia, and hypochondriasis, and the presence of oxalic acid in the urine. For, first, intense oxaluria may be present without evoking a single symptom of the kind; and, secondly, all the above symptoms may show themselves in their most aggravated typical form without a trace of oxalic acid in the urine. Constant and careful examination of the urine shows that almost every variety of disease is occasionally associated with oxaluria. Thus, Roberts has found it in phthisis, morbus cordis, emphysema, chronic rheumatism, malignant disease of the stomach, chronic vomiting. It cannot, however, be denied, that, when persistent and excessive, oxaluria is a serious condition, but serious as leading rather to the formation of calculus than implying the existence of any diathesis.

Notice is taken of the *pigment bodies* which must have been observed by every one who is in the habit of examining urine microscopically. They appear as amorphous particles of various sizes, reddish-brown or orange in colour, and as cellular but flattened ovoid bodies, called "celloids," in which are collected particles of pigment like the above. Their presence is not explained; but bodies precisely similar are stated to have been found in the brain near apoplectic clots, in old extravasations, in encephaloïd and other tumours. Heller's plan of testing for albumen is the one always practised and recommended by the author, the only difference being that the author uses the ordinary test-tube, whereas Heller employs a small wine-glass, which is preferable. A small quantity of the urine to be examined is poured into the wine-glass; and into the latter, slightly tilted so as to allow the acid to run down in close contact with the side of the glass and not diffuse itself through the urine, is poured a drop or two of nitric acid. The acid, provided that it be thus carefully poured, sinks to the bottom of the glass, and forms there a slightly coloured ring. If albumen be present, even in the smallest quantity, it is precipitated, and lies *in immediate contact with, and directly above,* the ring of acid. If urates be precipitated, they do not lie thus in direct contact with the acid, but form an irregular cloudy deposit above, *a clear layer of urine being always interposed between them and the acid.* If, as often happens (in the early stages of fever, pneumonia, &c.), both albumen and urates be precipitated (Heller's "doppelte Reaction"), there are seen, first the coloured ring of nitric acid, immediately above this the layer of albumen, then a clear layer of urine, and above this a cloudy deposit of lithates. We do not hesitate to affirm that albumen, even when present in the smallest quantity, is more easily and more surely discovered by this plan than by any other.

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[1] Although the flow of gastric juice into the stomach commences as soon as food is swallowed, yet it must be borne in mind that this process is a gradual one; that probably the height of acidity in the stomach is only reached at a late period of digestion.

Fehling's standard solution is the only positively trustworthy test for sugar. The precaution of boiling the test should be taken before adding the urine, in order to show whether the solution is pure. It is apt to spoil by keeping, in which case the suboxide is precipitated at once on boiling.

Part II deals with "urinary diseases," in which an abnormal condition of the urine is present, not dependent on any recognised structural change in the kidneys. His observations, based on an analysis of seventy-two cases, lead him to the conclusion that *diabetes insipidus* is a neurosis having its primary cause in some disorders of the sympathetic system. Bernard's experiments support this view of the question. Again, eleven of the seventy-two cases were distinctly traced to injury or disease of the brain. Lastly, the polyuria not infrequently disappears for a while, and then reappears, more after the manner of a nervous affection than of any organic disease. Next follows a very good résumé of the physiology and pathology of *saccharine diabetes,* in which due weight is given to the teaching of

Pavy and McDonnell; while Schiff's remarkable discoveries are also brought prominently forward. It is now accepted by the best authorities that diabetes depends proximately on some disturbance of the amyloïd substance of the liver as regards its destiny and function; but that this disturbance may originate away from the liver in the nervous system, where the seat of the lesion is in some cases clearly discoverable.

On the *medical or solvent treatment of calculi* Dr. Roberts is qualified to speak with authority, as he has for years past made it the subject of special investigation. The calculi open to attack are, on the one side, uric acid and cystine, which are soluble in alkaline solutions; on the other, phosphatic calculi, which are soluble in acid solutions. By subjecting uric acid calculi out of the body to the action of a continuous stream of an alkaline solution, or of urine alkalized by the administration of acetate or citrate of potash, he has arrived at the following conclusions:—First, that a solution of carbonate of potash has a more powerfully solvent action than one of carbonate of soda. Secondly, that in order to be of the proper strength, the solution must contain from 40 to 60 grains (neither above 60 nor below 40) of alkali to the pint of water. Thirdly, that a solution of maximum power will thus dissolve from 10 to 20 per cent. of a calculus in twenty-four hours. Fourthly, that repeated doses (Potass. Acet. gr. xl, vel l, ex Aq. ℥iij, 3â quâq. horâ) of acetate of potash maintain the urine at the best strength for the solvent purpose. Fifthly, *that urine alkalized in this way and passed over the surface of a uric acid calculus at blood-heat will dissolve the calculus at the mean rate of* 12⅔ *grains in the twenty-four hours.* Sixthly, that this treatment is inapplicable to all cases in which the urine is alkaline. Seventhly, that when the urine is acid this treatment is *primâ facie* suitable, unless abundant sediment of oxalate of lime or the previous passage of oxalate calculi lead to the supposition that the calculus is composed mainly of oxalate, in which case treatment is hopeless. Eighthly, that in the case of renal calculi five sixths of which are composed of uric acid, the patient should have the benefit of a doubt and the treatment should be tried; for, even if unsuccessful, the administration of the alkaline salt, as above, is perfectly harmless. Ninthly, that if the stone is known to be large, treatment is useless. With regard to the practical working of the solvent plan, instances of its success are not as yet, we think, sufficiently numerous or convincing to ensure its general adoption. He himself narrates only one case in which renal calculus was *probably* present and which terminated favorably after his plan of treatment. Three cases of vesical calculus seen and treated by him are also related, in one only of which is there satisfactory proof that a solvent effect had to a certain extent been produced on the calculus, which was afterwards removed by lithotomy.

The acid treatment of phosphatic calculi by the injection of weak solutions of nitric acid (Acid. Nitr. dil. ʒij. ex Aq. Oj), first proposed by Brodie, has been tried by Roberts, and is, he thinks, capable of wider application.

The urinary disease known by the name of *chylous or lymphous urine* depends most probably, as Carter has suggested, on a "leak in the lacteal system," by which a direct communication is established between some part of the lacteal or lymphatic system and the urinary passages.

Part III contains an account of *organic diseases of the kidneys*. Congestion of the kidneys occupies the first chapter, and in connection with this disease the *albuminuria of pregnancy* is fully discussed. Dr. Roberts' opinion as to the causes of puerperal convulsions does violence to neither of the pre-existing theories on the subject, but accepts both. Thus, he recognises —1, cases which depend on confirmed and chronic Bright's disease, where the convulsions are purely uræmic; 2, those which are caused by passive congestion of the kidneys, in which the convulsions are partly uræmic, partly reflex; 3, which are the result of reflected uterine irritation and are not attended with albuminuria. In the first only of these three classes does he recommend the inhalation of chloroform, while in the second and third active depletory measures are, he thinks, indicated.

A clinical classification of the different renal affections included under the common head of *Bright's disease* is preferred to the more scientific classification which is based on morbid anatomy; but it will be seen that the author's classification is really a mixed one, partly based on clinical symptoms, partly on morbid anatomy. Thus, he recognises A, acute Bright's disease (the "desquamative nephritis" of Johnson and "acute tubular disease" of Dickinson); B, chronic Bright's disease, subdivided into—a, the smooth white kidney; b, the granular red kidney; c, the lardaceous or waxy kidney. These three states he regards, in common with Johnson and Dickinson, as purely distinct diseases. The minute changes of structure in the kidney, the symptoms, and the peculiarities of the urine in each of these different states, have been so clearly put forth by the above writers, and are so well known to all, that it is needless to repeat them here. It may, however, be mentioned that fatty degeneration of the epithelium or matrix is not considered by Roberts to be special to any one type of renal degeneration, but may, he thinks, be associated with any of the above conditions. The old term "waxy" or "lardaceous," as applied to the third subdivision, is purposely used in preference to the new, and now generally adopted, adjective "amyloid." Since this substance has been shown by Kekule and Schmidt, when separated and examined chemically, to contain as much as 15 per cent. of nitrogen, and to resemble albuminous rather than starchy compounds in its behaviour with reagents, the word amyloïd, invented by Virchow, ought to be abandoned, though the epithet 'waxy,' or 'lardaceous,' is also infelicitous.

The last chapters in the volume, those more especially which treat of hydronephrosis, cystic degeneration, cancer, and tubercle, add very materially to the value of the work as a whole, for the reason that they give a full account of some diseases which are clinically most important, but to which very little attention has been paid by writers on renal and urinary diseases. Thirty-seven cases of *hydronephrosis* (distension of the kidney into a large sac filled with its own secretion) are collected. In fourteen of these, an obstruction to the escape of urine with consequent distension behind the seat of obstruction was caused by congenital malformation of the kidney, ureter, or renal artery. In eight, the mischief was the result of a calculus impacted in the ureter. In four, constriction from inflammatory thickening of the ureter was the direct cause. In six, the ureter was occluded near its entrance into the bladder by pressure of a cancerous or other tumour. In one case the tumour was successfully tapped (Thompson, 'Path. Soc. Trans.,' vol. xiii), the part chosen and recommended for the operation being the interval between the last two ribs near their anterior extremities.

The cause of nearly all cystic degeneration of the kidney, no matter what the size of the cysts, seems to be dilatation of one of the convoluted uriniferous tubes in consequence of closure of the tubule in some part of its course. A cyst of the kidney is, in fact, a localised hydronephrosis, the principle of the formation being in both the same. Virchow has clearly shown how congenital cysts are caused by obstructions in the straight tubules; and there can be no doubt that the tendency of modern inquiry is to limit and simplify the explanation of this phenomenon, and to do away with the theory of Rokitansky and others that renal cysts are monstrous nuclei.

Whereas *cancer* is present almost invariably in one kidney only (in 47 out of 53 cases recorded), *tubercle* most frequently affects both kidneys (in 17 out of 28 cases). In cancer a tumour of greater or less size is invariably present, and hæmaturia occurs in one half of all cases. This latter statement is based on an analysis of 49 cases, in 24 only of which hæmaturia was present. He is very particular in laying stress on this point in diagnosis, that hæmaturia is not a constant symptom; whereas Basham evidently regards it as a most constant symptom, when he says ('On Dropsy connected with Disease of the Kidneys,' &c., Ed. 2, p. 332), "malignant disease always accompanied by violent and repeated hæmaturia." Cancer, when occurring in the kidney, runs a more chronic course than in any other internal organ, its average duration being two and a half years. Roberts is here at issue with Walshe and Lebert. The symptoms of tubercle are mainly those of chronic pyelitis, with, as the disease gains ground, emaciation, hectic, and, in nearly all instances, secondary tubercle in some other organs, particularly the lungs and intestines. Whereas in cancer the urine rarely contains pus, often blood, in tubercle the presence of pus is the rule, of blood the exception.

We cannot conclude this short sketch of a good book without recommending it strongly to medical men as an excellent digest of recent work in the department of urinary and renal pathology, and as a truly clinical guide disencumbered of all unnecessary chemical details.

REVIEW III.

A Proposal, in order to Diminish the Progress of the Distemper among Horned Cattle, supported by Facts. By MALCOLM FLEMMING. *London.* 1755.

An Essay on the Nature, Cause, and Cure of the Contagious Distemper among Horned Cattle in these Kingdoms. By PETER LAYARD. *London.* 1757.

The Three Reports of the Commissioners appointed to inquire into the Origin and Nature &c. of the Cattle Plague, with Appendices. (Presented to both Houses of Parliament, by command of Her Majesty.) *London.* 1866. Pp. 199—81—244.

The Cattle Plague; with Official Reports of the International Veterinary Congresses held in Hamburg, 1863, *and in Vienna,* 1865. By JOHN GAMGEE, Principal of the Albert Veterinary College, London, &c. *London:* Hardwicke. 1866. Pp. 859.

On the Cattle Plague, or Contagious Typhus in Horned Cattle: its History, Description, and Treatment. By H. BOURGUIGNON, Doctor of the Faculté de Paris, &c. *London:* Churchill and Sons. 1865. Pp. 379.

IN the month of June, last year, a disease new to the experience of all but one or two of our native veterinarians broke out among the dairy cows in London, and rapidly spreading amongst the herds of cattle throughout Great Britain, assumed, in the course of a few months, the proportions of a national calamity. For lack of a better designation, it has been known as the "cattle plague," and it was, notwithstanding some varieties in its symptoms, early recognised by Mr. Symonds, of the Royal Veterinary College, as identical with the epizoötic malady known on the European Continent as the rinderpest, or Russian Steppe murrain, as well as by a variety of other names, all more or less suggested by theories held by their several authors as to the nature of the disease. It will be a wiser course, at present, to employ only the trivial designation of the malady—which, indeed, has now become a household word amongst us — than to hamper ourselves with any name implying an amount of knowledge which we do not possess. The English literature of cattle plague, until last year, was scanty—happily so, in one sense, since the latest authors, who had written from their own observation of the disease in this country, date as far back as 1757. We have placed the titles of two of the most important of our older treatises upon cattle plague at the head of this article. But what was wanting in our native literature is amply compensated for by the abundance of foreign treatises; while foreign, and especially the German, medical and veterinary periodicals absolutely teem with the contributions of men who have had but too good reason to devote time and labour to investigations into its nature and its mode of prevention. Upon the latter point Continental writers are tolerably agreed; but the measures which would have been inevitably adopted in a Continental State, had a similar event happened to that which occurred to us in July last year, were not adopted here. Let us add that, putting aside the paralysing effects of the first surprise, and the natural scepticism with which the announcement of the new disease must have been met at head-quarters, it would have been inconsistent with our national arrangements to have adopted them. We do not propose here to discuss any of the political questions that have arisen as to the action the advisers of the Crown might or ought to have taken when first they learned from Mr. Symonds the character of the new disease. The steps which they did take—the only steps which they considered they were at liberty to take—are too well known to require enumeration. That their powers were no larger was, perhaps, unfortunate; we had hoped that Parliament would have extended them last session, but that hope was disappointed. However, they did one good thing, which goes far to induce scientific men, at all events, to condone any faults which may have preceded. They determined to inquire into the character of the disease for themselves; and this inquiry, committed to men high in scientific attainments or in practical knowledge of stock, with Dr. Bence Jones as their chairman, has resulted in a report to Her Majesty which reflects the highest credit upon all concerned in its production. The extension of our knowledge has, indeed, been dearly purchased: experience never yet was cheap; the advantage of knowledge thus acquired, however, is, that it is fruitful in practical results. May it be so on this occasion! When next taunted with our insularity, we may point with confidence and pride to the admirably conducted investigations, the results of which are contained in the Appendix to the 'Third Report of the Cattle Plague Commission.' "Young Physic" was suddenly put upon its trial; it has passed through the ordeal triumphantly, and has proved that its training has not only qualified it for investigations in human pathology, but has fitted it also to give authoritative counsel upon matters hitherto popularly supposed to be beyond or beneath its cognisance. In the remarks we are about to make, we propose to keep in mind the practical objects which the Commission sought to attain; at the same time, we must not lose sight of the fact that the scientific investigations made upon the cattle plague have a most important bearing upon the pathology, prevention, and treatment of these diseases of the human frame with which we are most closely concerned, and especially upon the pathology and prophylaxis of the class of diseases to which cattle plague appears to belong.

I.—Cattle plague is a specific disease capable of communication by inoculation in various ways; and perhaps the best mode of describing its phenomena will be to mention in order those which are observed in a typical case following inoculation. The first sign that the virus is operating is afforded by the thermometer introduced into the rectum or the vagina. The credit of this discovery, one of great importance in respect to the probable results of future abortive treatment, is due (in England) to Professor John Gamgee; and his observations have received full confirmation from those of Dr. Sanderson. The natural temperature being about 102° Fahr., it is found, in about thirty-six or forty-eight hours after inoculation, to have risen to 104° or 105½°; and this at a time when no other indication is observable that the animal is in any way ill. This symptom, then, marks at forty-eight hours or thereabouts the termination of the incubative stage, and the true commencement of the disease. In about two days from this time a peculiar eruption is noticed in the mouth, and almost simultaneously a very distinctive appearance on the mucous membrane of the vagina. On the next day, the animal looks a little ill, has less appetite, and ruminates irregularly; and on the following day, the fourth from the true commencement of the disease, it shows such marked symptoms of illness as could not escape the notice of the most superficial observer. These symptoms, popularly, mark the beginning of the disease, which had truly commenced about 110 hours before. The head hangs down, the ears are thrown back, and the attitude and movements are suggestive of depression; the breathing mostly becomes oppressed and irregular, sometimes with cough; there is a discharge from the eyes and nose, first glairy, afterwards turbid, and the pulse is hard and threadlike. About the sixth day, when the alterations of the mucous membrane of the mouth have attained their full development, there is usually a crust of opake white material over the under lip and other parts of the mouth, separating on the slightest touch and exposing the red vascular surface of the membrana propria beneath: about this time also, with increased feebleness, the constipation which existed previously is succeeded by diarrhœa, and sometimes there is subcutaneous emphysema; the temperature sinks, and death usually occurs during the seventh day.

We may proceed to fill in this outline. We have already referred to the early *rise of animal temperature.* The practical importance of watching for this phenomenon in a suspected herd is illustrated by Professor Gamgee in the instance of a farm near Lanark, where, on examining forty-two cows, several of which were eating and ruminating, and others giving a full quantity of milk, he found all with the exception of one exhibiting a temperature varying from 104° to 107·8°. This was on the 18th of November; only five of the whole herd were alive by the 25th. Little reliance can be placed upon the indication of the *pulse,* except as regards its thready character. The acceleration in the early stages is very slight, and in one case Dr. Sanderson found the pulse slower than in health; of course it is accelerated during the final collapse. Much care is necessary in studying the respiration; and here, again, the *mode of respiration* is more important than its frequency. The trifling irregularity observed on the fifth day assumes on the sixth "a character which is so remarkable, that if once observed it cannot be forgotten." A sudden inspiratory movement is immediately followed by a closure of the glottis, attended with a sound so loud as to be heard at a considerable distance, the whole body of the animal being often thrown into oscillation by the resisted expulsive effort: after a rest of one or two seconds the air is expelled with a peculiar grunting noise. Dr. Sanderson compares the mode of breathing to that observed in animals whose pneumogastric nerves have been divided. The same result occurs frequently in both instances, viz., *emphysema*, the first evidence of which is the extension of it to the cellular tissue of the back, which the escaped air reaches from the interlobular cellular tissue of the lungs by way of the posterior mediastinum. Simultaneously with the rise of temperature—and, indeed, rather earlier—there is generally an excessive secretion of *urea* by the kidneys; a fact which, taken in association with the well-known observations of Drs. Parkes and Ringer, goes far to establish the connection between cattle plague and the class of "Fevers." The secretion of *milk* is in cows arrested, and the milk itself undergoes changes fully described by Dr. Marcet and Dr. Beale in their several reports. With respect to the *alvine dejections*, the reporter of the Commissioners, Dr. Sanderson, generally confirms the observations of Spinola, that on the third or fourth day (from first rise of temperature) there is usually constipation, the fæces being firm and dark, and invested with a covering of blood-stained mucus. After a day or two this is succeeded by diarrhœa, with pain and tenesmus: in many of Dr. Sanderson's cases it did not set in until the last or penultimate day. The discharges become more and more profuse, and acquire a putrid offensive smell; in the last stage they may resemble pea-soup or rice-water, or consist only of mucus and blood. Dr. Sanderson has never observed any evidence that cattle plague interferes with the *functions of the nervous system*; and especially three symptoms described, and apparently with truthfulness, by Continental writers, have been absent in the cases he has closely observed in this country. These symptoms are, rigors or convulsions, lumbar tenderness, and delirium. The truth appears to be, from all we can gather, that the disease varies a good deal both in severity and in symptomatology with locality and breed. Professor Jessen stated at the International Veterinary Congress held at Vienna last year, that the disease in the Steppes "may appear in so mild a form, that no veterinarian, be he ever so experienced, will be able to recognise it;" and his assertion received confirmation from Professor Röll and others who were present; while Dr. Sanderson quotes the experience of Haupt in Russia, Seer in Bohemia, Ritter von Koch in Austria, Spinola and Weber, in evidence that the symptoms we have stated to be wanting generally here are, one or other of them, characteristic symptoms in their observation.

We have reserved to the last the consideration of two classes of symptoms which are in a pathological point of view more important than any,—those which relate to the condition of the mucous membranes within reach of observation during life, and those which relate to the condition of the skin. First, then, as to the *mucous membrane of the mouth*,—alterations in which were noticed in all but one out of seventy cases of cattle plague observed by Dr. Sanderson. These alterations were observed earliest just inside the mouth, consisting first of minute elevated points, about the size of poppy-seeds, either upon that portion of the gum which covers the socket of the corner tooth on each side of the lower jaw, or scattered over the upper gum or dotting over the inner surface of the under lip. In general, this eruption was not preceded by any increased redness; but in a few instances out of those closely observed from the commencement of the disease, there was some reddening apparent for some hours, or even a day, previously. These granulations coalesce, so that the surface appears during the third or fourth day as if covered with a curdy or creamy deposit almost resembling a false membrane. In a day or two after its formation this creamy concretion exfoliates, leaving exposed, wherever this happens, the bright red surface of the membrana propria, thus giving rise to the appearances which have been designated "pestilential erosions of Kausch." A day or two after their first appearance, similar alterations are observed on the lining membrane of the cheeks, on the tongue, especially on the under surface near the borders, and on the hard palate. As might be expected, there are exceptional appearances and varieties, which are described by Dr. Sanderson. In animals that recovered, it was observed that the gums and lips rapidly resumed their natural appearance. Next, as to *the mucous membrane of the vagina:* here increased intensity of colour appeared to precede other alterations which were similar to those observed on the gums; the surface was sown over with grayish white elevations not so large as pin-heads, and was abundantly covered with discharge, creamy, or yellowish and sticky, or it presented red patches of excoriation. "I have never seen," says Dr. Sanderson, "on the vaginal mucous membrane any appearance which could be compared to a vesicle." In this description all the reporters to the Commissioners agree, but the unanimity is not quite so perfect in regard to the *condition of the skin.* We shall here, then, merely describe what Dr. Sanderson observed during life, leaving the consideration of the nature of what he observed to a future discussion. He divides the appearance on the skin into "*incrustations*" and an "*eruption.*" The incrustation he states to consist in "the exuding of a semi-solid material on the surface of the skin, whereby it is rendered unctuous to the touch." This occurs about the fourth or fifth day, on the insides of the thighs, in the immediate neighbourhood of the anus and vulva, and on the chin; and it concretes into a dry crust of a pale or canary yellow colour, differing in appearance according to situation. On the nucha and sides of the neck he describes it as assuming an appearance like that of eczema on the human scalp. Murchison also describes patches of roseola or erythema which terminate in furfuraceous desquamation. The furfuraceous desquamation upon a slightly rosy surface is also mentioned by Dr. Bristowe; but Dr. Sanderson says, "I have, however, seen no indications of reddening of the surface (roseola) excepting on the udders of milch-cows, and in this situation only at a period shortly preceding death." He describes, however, as one variety of "eruption," a thickening, softening and ultimate detachment of the crumpled epidermis in the perinæum and on the udder of cows, and incorporated with the incrustations both here and about the mouth. The *elevations* Dr. Sanderson has observed are of three kinds:—1. Soft papular eruptions, described by various authors, such as Knötchen and Höckerchen. 2. Hard crusted elevations, somewhat resembling impetigo pustules, and met with only on the udders of milch-cows. 3. Soft tubercles of larger size than the first, due to the enlargement and inflammation of sebaceous follicles. Neither he nor Dr. Murchison regard the last as having any special relation to cattle plague.

II. Passing over for the present the structure and exact nature of these elevations and other appearances upon the skin and mucous membrane, we may briefly narrate the results of observations made upon other parts of the carcases after death. This part of the inquiry was placed by the Cattle Plague Commissioners in the hands of Dr. Bristowe, who details at length and tabulates the observations he made upon thirty-five carcases of dead cattle, and eleven sheep, goats, and deer. The general condition of the mucous membrane was one of congestion, passing on to hæmorrhage into the sub-mucous tissue, sloughing, and subsequent ulceration. The *nasal membrane* was generally nearly black with congestion; and the *conjunctivæ* of the eyes also were almost invariably congested, with some secretion upon their surface, but no further change was met with in connection with the eyes. The congestion almost invariably extends along the *pharynx*, and the first two or three inches of the *æsophagus*, these parts being also usually studded with minute granular spots and excoriations. The *rumen* is usually full of food, as might be expected from the arrest of rumination; and although in most instances the mucous membrane is healthy, yet sometimes congestion and even sloughing are observed. The *reticulum* is also usually healthy; Dr. Bristowe has never noticed anything beyond congestion, and that in only two or three cases. The contents of the *omasum* are almost invariably dry and caked, and moulded to the surface of the folds of this stomach. Although sometimes healthy, there is usually congestion of the mucous membrane, passing on to gangrene, and in protracted cases to separation of the slough and cicatrization. It is remarkable that cicatrization may take place even when the disease is hastening to a fatal issue. The microscopic fungi which Dr. Bristowe describes as present on the sloughs, and which Dr. Beale also recognised generally in the mucous discharges, do not appear in any way essential phenomena in this disease. The *abomasum* generally, but by no means necessarily, exhibits evidences of congestion, extravasation of blood, sloughing, and excoriatious; the extravasation commonly exhibits an appearance like that of petechiæ or flea-bites; the sloughs and excoriations as a rule chiefly abound in that part of the stomach which lies between the folds of the mucous membrane and the pylorus. Congestion, extravasation of blood, sloughing, and ulceration are also observed in the *intestines*. The parts most commonly thus affected are the small intestine or some part of it, the cæcum and ascending colon, and the last five or six inches of the rectum, the last-mentioned part being always more or less deeply injected, more deeply and more uniformly so than any other part of the large intestine: the submucous hæmorrhage may occur in the form of ecchymoses or petechiæ, and, as is the case also with the stomach, blood may even be poured into the cavity. In the small intestine, the hæmorrhage is most frequent in the duodenum. In Dr. Bristowe's experience, sloughing and ulceration occur but rarely in the intestinal canal; but, of course, the morbid conditions in different cases vary much in extent and degree. One important point to have established is the freedom of Peyer's patches from any special lesion—from any change at all resembling that which is observed in the enteric fever of man. In some cases, indeed, they are less prominent than in health. In this absence of lesion of the glandulæ agminatæ all our observers are agreed, both those who have reported to the Commissioners, and a number of independent observers, such as Dr. Aldis, Mr. Burge, and Dr. Crisp; and Mr. Gamgee also expresses, in his work, his concurrence with this statement. The morbid conditions which may in certain cases be observed in the glandular structures, and which are described much in the same way by German writers as by our own observers, are not peculiar to cattle plague. The *liver*, *pancreas*, and *spleen* present no special evidences of structural alteration. Passing on to the respiratary organs, similar conditions of the mucous membrane are observed to those noted in the alimentary canal. The *larynx* is usually congested in its whole extent, and there are more or less œdema and swelling of the submucous tissue, especially of that connected with the epiglottis and aryteno-epiglottidean folds; and also there is more or less submucous hæmorrhage. The mucous surface is often partially covered with a slightly adherent layer of what Dr. Bristowe regards rather as inspissated mucus than true false membrane, sometimes even covering the vocal chords, and entangling small clots of blood. Congestion and submucous hæmorrhage are generally observed in the *trachæa,* the upper part and the lowest extremity being the parts most commonly thus affected; and the upper part of the membrane may be covered with the same sort of exudation as that observed on the larynx. The bronchial tubes are liable to present similar appearances of congestion and hæmorrhage, sometimes, probably from hypostatic causes, one lung being more diseased than the other. They may contain frothy or muco-purulent fluid or coagula of blood, the latter occasionally moulded into the cavities. With the exception of their being emphysematous, the *lungs* themselves are generally healthy; true pneumonia is a rare complication. The emphysema is interlobular, and in some degree is found in nearly every case of cattle plague; sometimes it is so extensive as to divide the whole tissue of the lungs into a series of lobules, and to pass into the subpleural tissue. When it is at all abundant, the extravasated air distends widely the tracts of interlobular tissue which radiate from the root of the lung, and passing into the mediastinum, extends from them to the tissues about the kidney and the subcutaneous textures of the back. The interlobular character of the emphysema is due to the anatomical peculiarities of the lung of the cow, where the interlobular cellular tissue is abundant and where rupture of air-cells readily permits the passage of air into it. The only characteristic morbid appearance about the *heart* consists in a few petechial spots occasionally found scattered about the internal surface, especially at the base, and effusions of blood beneath the lining membrane of the left ventricle. The latter is the most common and characteristic appearance, the effusion being most constant and largest about the apices of the musculi papillares. The *blood* itself is somewhat darker in colour than in healthy beasts, and, as a rule, coagulates less quickly, but the microscope detects nothing abnormal. Dr. Bristowe says that the *kidneys* present for the most part a healthy aspect, and only now and then are found congested; and that they are at least as normal as they are in typhus, diphtheria, &c., in the human subject. There is reason to believe, however, that there is often at an early stage intense hyperæmia: such is the statement of Dr. Murchison, and it corresponds with the results of Dr. Beale's microscopical researches. From the appearance of the Malpighian bodies, this observer concludes that their capillaries would at first be highly congested, but that afterwards the congestion would be to some extent relieved by the escape of blood, serum, and corpuscles into the urine. The lining membrane of the *bladder,* often normal, is sometimes congested, and presents spots of submucous hæmorrhage. The same condition is sometimes observed in the *urethra.* The *uterus* is sometimes a little congested, especially at its neck, where there may be a little submucous hæmorrhage. A fœtus if found within it has evidently been dead for some time, being soft and decomposing, but free from disease. Very commonly there are congestion and submucous hæmorrhage in the *vagina,* especially near the *external urinary meatus* and *vulva,* which present the same appearances. The *nervous centres* and *serous membranes* are mostly quite healthy. As regards the relative frequency of the appearances of which we have given a brief abstract, Dr. Bristowe says:

> "To speak generally, the visible effects of the cattle plague are manifested chiefly in connection with the cuticular and mucous tissues; and of these the disease seems chiefly to select the skin and the mucous membranes of the alimentary canal and air-passages, including that of the pharynx, mouth, and nose. One or more of these tracts may occasionally escape wholly, the others being at the same time seriously diseased; but in the great majority of cases all of them become affected during the progress of the malady, though which of the several tracts and which particular parts of them shall suffer most severely is a matter of uncertainty. The mouth and fauces are generally affected, and affected simultaneously in all those parts which I have pointed out as chiefly liable to suffer. Of the air-passages, disease is most common in the larynx and in the trachea; and of the alimentary canal, the parts most prone to suffer are the omasum, abomasum, small intestine, cæcum, and ascending colon and rectum. Of these the abomasum is certainly most frequently involved. Another appearance in the disease, as constant nearly as those which have been enumerated, but far less important, is that of ecchymosis beneath the lining membrane of the left ventricle of the heart. The genito-urinary mucous membrane seems never to be very seriously affected: morbid processes take place, however, more generally in the lower part of the vagina and vulva than elsewhere; and from the facility with which these parts can be investigated, their condition furnishes often a valuable aid to diagnosis. The lungs, with few exceptions, become more or less emphysematous. It has seemed to me that in cattle plague, disease of internal organs is often aggravated and may be determined by mechanical and other accidental causes."—*Third Report of Commissioners,* p. 92.

III. The *diagnosis* of cattle plague when the disease is advanced is a matter of no difficulty; the most important thing is to determine its nature in the earliest stage, and (if the disease be curable) in the stage when remedies are most likely to be successful. It is here that Professor Gamgee's observation of the rise of animal temperature is most valuable; and although it is likely that in other febrile and contagious diseases of cattle a similar early rise of temperature might be detected, yet where an animal is suspected or known to have been exposed to the contagion of cattle plague, the veterinarian who neglected to act upon this indication of invasion of the system would be inexcusable. The appearance of the characteristic eruption a day or two later, in the mouth and vulva, or in either separately, would suffice to confirm an opinion formed from the elevation of temperature, and to determine finally the nature of the malady. With regard to the subsequent discharge from the eyes and nose, Professor Gamgee gives a caution which is worthy of notice, and it has its bearing upon the evidence addressed before the Commission respecting the disease reported to have been observed among the Revel cargo of cattle, which is supposed by some to have introduced the cattle plague into this country in June.

> "No symptoms can better illustrate the care required in diagnosis than the discharge both from the eyes and nose. The French Commission conducted a series of experiments in my establishment in Edinburgh; and great care was exercised in purchasing animals free from rinderpest, especially for inoculation experiments. Three animals were bought one day; and all had a glairy discharge from the eyes and nose. The person who had procured the animals was certain of their being free from plague, and the herd from which they were obtained was inspected. All, without exception, had the discharge. They had been exposed to easterly winds in an ill-sheltered field, and were suffering from a slight catarrh." (P. 47.)

With regard to *prognosis*, certain important points are made out. When the lesions of the alimentary canal, and especially of the respiratory organs, are considered, no one can wonder at the fatality of the disease; but even in the worst cases, as has been mentioned, there is evidence of considerable restorative power in the fact of cicatrization being found in progress even at a time when sloughing is going on in another part. In animals about to recover, improvement generally commences about the seventh or eighth day; but convalescence is slow; the peeling off of the incrustations is accompanied by itchiness, and occasionally there is a copious discharge of urine. Dr. Bristowe and Dr. Sanderson both remark how rapidly, when convalescence sets in, the mouth and other organs regain their original healthy condition; so rapidly, that in twenty-four hours newly exposed surfaces, red and raw-looking, assume an appearance so natural that they can hardly be distinguished from unaffected parts. Dr. Sanderson also noticed, that in animals that recovered the diarrhœa which succeeded the constipation was of short duration, and that the evacuations never acquired dysenteric characters. Some indication of the probable course of events, too, may be gathered from the temperature and pulse. As to the former, Dr. Sanderson observed that in his inoculated cases the acme of exalted temperature was reached in the fatal cases a day earlier than in the others, and that the decline was rapid and decisive, while in the animals that recovered the decline was slower and more irregular. It is further remarkable that the danger does not appear to be proportionate to the elevation of animal heat, since he observed that the cases in which the standard of health was most exceeded were precisely those in which the issues were most favorable!

> "If the heat of the body begins to sink rapidly during the fifth and sixth day, the change indicates with tolerable certainty that death is approaching. In this case it is accompanied or followed by a marked diminution in the force of the pulse, by an aggravation of that peculiar dyspnœa which is observed in almost all severely affected animals, and in some cases by a sudden aggravation of the diarrhœa. . . . In animals about to recover the temperature also falls; but the decline is much more gradual, and does not commence until the seventh or eighth day. It is coincident with the gradual restoration to health." (P. 4.)

We have before noticed that the frequency of the pulse is not important as regards diagnosis. In the collapse which precedes death it is, as might be anticipated, the acceleration more marked than at any other period. It is remarkable that in one of Dr. Sanderson's animals which recovered, the pulse was actually slower by two and a half beats than in health.

V. The pathology and alliances of the disease may next receive attention. Cattle plague is a febrile disease that is capable of being communicated from the sick to the healthy by inoculation. This is a fact which has long been known, and it suffices to establish to a certain extent its nosological position. The matters commonly employed for producing the disease are the discharges from the nose and eyes, and the serum of the blood. In four of Dr. Sanderson's cases the serum for inoculation was taken from a cow seventeen hours only after the first rise in temperature, and therefore before any of the ordinary symptoms of ill-health were observable. Here, as the Commissioners observe, is a "most important pathological discovery."

> "It is pregnant with consequences in medical doctrine; for though the existence of a similar fact has been long suspected in several human diseases, it has never been proved in any. So material, indeed, is it, that we must dwell on it a moment. The poison con-

tained in a minute portion of the mucous discharge from the eyes and mouth of an animal ill with cattle plague, if placed in the blood of a healthy animal, increases so fast that in less than forty-eight hours—perhaps in a far shorter time—the whole mass of blood, weighing many pounds, is infected; and every small particle of that blood contains enough poison to give the disease to another animal. This at once accounts for the rapid spread of the cattle plague. The agent is multiplied to a large amount in a very short space of time. How soon after the poison is put into the blood the animal becomes capable of giving the disease by natural infection to other animals is not determined; possibly not until those parts of the body which can give off products to the air become impregnated with the poison. At what time the blood and textures cease to be able to give the disease is also not determined." (P. 4.)

What is the nature of the contagious poison—the "virus," as Mr. Crookes prefers to call it? A most important question for determination, since whatever be the nature of the contagious matter—*contagium,* as Mr. Simon designates it—in cattle plague, that, *mutatis mutandis,* is the nature of the contagium of small-pox, scarlatina, measles, typhus, &c., in man. The opportunity for investigation has not been lost, and we owe to Dr. Beale as near an approach to the question as we are likely to attain until our acquaintance with the nature of *life* and its relation to chemical force is vastly improved beyond what it is at present. Using the highest magnifying powers that exist—powers of $\frac{1}{50}$ in. focal adjustment, he has ascertained the following facts:—1. That generally throughout the body and diseased textures there is a great increase of nuclear matter—or, to use Dr. Beale's favorite term, "germinal matter"—both within and outside the vessels. He finds this abundant increase of germinal matter in the discharges, in the small veins and capillaries, in the ganglion cells and plexuses of nerves which were some years ago demonstrated as lying beneath the mucous membrane of the intestines, and in the blood wherever stagnated at the time of death. Regarding with other previous authors[1] the white blood-corpuscles as consisting of agglomerations of this material, he states that they are greatly increased in number and size, and that sometimes a capillary vessel appears to be occupied with them and their descendants only. The accumulation of this germinal matter in the walls of the capillaries and in their interior cannot but interfere with circulation and nutrition in parts where it takes place, and assist materially in bringing about the local congestions observed. By its increase in the texture of the intestinal ganglia and plexuses their action would be diminished, and at length it is very probable that complete paralysis of the nerves of these ganglia which preside over the vessels, and probably to a great extent influence also the action of the muscular tissues of the intestine, would result. 2. He finds that the soluble elements of the blood are increased in amount; so that what with this, and the thinned and distended condition of the congested vessels, nutrient matter can pass more readily through them, thus affording pabulum for an increase in the germinal matter outside. 3. That the various masses of germinal matter, whether as found in the form of "white blood-corpuscles," "pus-corpuscles," or otherwise, possess an independent vitality, is demonstrated by the observation of their growth and multiplication, as well as by their exhibition of amœbiform movements—alteration of shape (like the amœba), when preserved under favorable conditions as to moisture and temperature. This observation is not absolutely new, but Dr. Beale goes a step further, and has demonstrated that these evidences of vitality are not confined to the masses of germinal matter, but to each individual particle of which they consist. He has demonstrated this fact in the instance of the minute particles of which a pus-corpuscle is made up, and has satisfied himself that the apparent alteration in form is not a mere result of rotation of an irregularly shaped particle, but an actual variation in shape. He believes that he has seen similar movements in particles of vaccine lymph. 4. There is the further fact, that no difference can be detected in the appearance of germinal matter as obtained from different sources; "a minute particle of the germinal matter of an amœba could not be distinguished from a portion of a pus- or mucus-corpuscle or white blood-corpuscle, &c." Thus much for observed facts: beyond this we pass into speculation, but speculation with sufficient basis to render it worthy of being dwelt upon. Does the "*contagium*," or *materies morbi* of cattle plague consist of such minute particles as those whose characters have been above very briefly described? Dr. Beale is disposed to answer this question in the affirmative. First, it must be received as deducible from evidence such as is summarised by Dr. Angus Smith, in his report to the Commissioners, that the *materies morbi* of cattle plague and allied diseases is *living* matter of some kind, and such matter, capable of transplantation, is found in the discharges of sick animals; matter consisting of particles so minute as to be capable of dispersion into the air, and of being wafted (volatile) by the wind to considerable distances. The difficulty that arises in conceiving such minute particles as have been described to be the matter of contagium, or the carriers of contagium, naturally consists in the impossibility of detecting any differences between particles from different sources: but, says Dr. Beale—

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[1] See an interesting paper on this subject in June and July Nos. of 'Archives Générales' of the current year.

> "If we carefully reflect upon many observed facts, we shall be compelled to admit that masses of germinal matter which resemble one another in every character we can ascertain differ nevertheless remarkably in *power*, as is proved by the results of their living. . . It need scarcely be said that physical and chemical properties do not determine the *form* living matter is to assume. . . The germinal matter of cuticular epithelium may give rise to the peculiar hard material of which the so-called walls of the epithelial cell consist; and this same germinal matter of epithelium, if freely supplied with pabulum, may give rise to pus. It seems to me, therefore, that germinal matter may *lose* formative power and become degraded, and cannot acquire it or regain it when lost. There is, as it were, no return to the high position for living matter which has once suffered degradation; nor can degraded germinal matter produce descendants with exalted power. . . . It would seem as if the formative or developmental endowments of germinal matter were diminished or completely destroyed by its rapid multiplication. . . . Ordinary pus then may readily be produced, if the nutrition of the germinal matter of a normal tissue be modified or increased. Under certain specific conditions which we are not yet acquainted with, pus with peculiar and specific properties or powers is formed; and this last exhibits a far greater vital activity and is less easily destroyed than the first. The various facts and arguments advanced in this report render it, I think, probable that the materies morbi or contagium of contagious disease, like pus, is generated in the organism under certain special conditions. Like pus, I think it has originally descended from the germinal matter of the organism." (P. 148.)

An analogue is found in vaccine lymph, where particles are discovered manifesting active molecular movements; and Dr. Beale says—

> "I should be no more inclined, in the absence of the most conclusive evidence, to regard the fluid portion of the vaccine lymph as the active material, than I should be to assume that the fluid in which the spermatozoa were suspended was the fertilising agent, and that the spermatozoa themselves were merely epithelial débris and quite unimportant; or that the fluid in which the yeast fungi or bacteria were growing was the active agent in exciting fermentation." (P. 149.)

That living germinal matter, supposing it to constitute or to contain the contagion, should be operative even after it has been dried, would be no more remarkable than that vaccine lymph retains its contagious qualities under similar circumstances, or that a rotifer or amœba should exhibit vitality again when remoistened after being dried. Supposing the contagion of cattle plague, &c., to consist of minute germinal particles, it would be quite possible that they should enter into the substance of normal living masses of germinal matter, such as are the white corpuscles of the blood. There is sufficient evidence from former observations, that white corpuscles as well as the amœba itself may thus receive by inclusion other matters into their substance, and in this way that these matters might be conveyed anywhere in the course of the circulation. Indeed, Dr. Beale seems inclined to believe that some particles that he figures may really be the particles of contagion; but whether this be the case or no, if we assume the contagion to be such as he supposes, there would be no difficulty in comprehending how it may gain access to the blood, when it comes into contact with any soft and moist mucous surface, as that of the lungs, &c., finding there a nidus suitable for lodgment and a pabulum fit for nutrition.

"In an animal about to be infected with cattle plague, it appears probable that the minute living particles having reached the surface of the mucous membrane, perhaps already rendered suitable for their nutrition by previous morbid changes, gradually multiply amongst the soft epithelial particles until some of those produced reach the walls of the capillaries almost immediately beneath. It is possible that some of them may be drawn down the windpipe during inspiration, or even into the air-cells of the lungs. In the latter case they would gain a very ready access to the surface of the capillary wall.

"We may suppose that at the time the particles come into contact with the vascular wall, the capillaries happen to be distended, and, consequently, their walls so thin as to permit small particles to pass through them. Such a state would obviously favour the entrance of minute particles into the blood. One can conceive them gradually extending, like particles of an amœba, and slowly insinuating themselves through the capillary wall; or they might become imbedded in the germinal matter of the capillaries, and in this way gain access to the blood. Having reached the interior of the vascular system, they would circulate with the blood, and for a time would give rise to no symptoms whatever; but, in consequence of possessing highly active powers of growth, they would grow and multiply, increasing at a greater rate as their numbers increased. They would soon derange the normal changes going on in the blood; many would probably soon become adherent to the wall as they traversed the more minute capillary vessels, and growing and multiplying in this situation, would seriously impede the circulation of the blood, partly, perhaps, acting mechanically—partly by deranging the nutritive changes occurring in the vessels and in the tissues external to them." (P. 153.)

The effect of the contagium, when operating fully in the system, is to produce locally the capillary stagnation and hæmorrhages already described. Dr. Beale does not regard the increased animal temperature as due to increased oxidation, since both respiration and circulation are often seriously impeded, but rather as connected with the increase of the germinal matter. "If this be so, it is probable," he says, "that an increase of germinal matter is *invariably* associated with the development of heat."

We may pass on now to consider the pathological alliances of the disease; a subject which is fully and ably discussed by Dr. Murchison, whose final opinion is endorsed by the rest of the reporters to the Commission. The human diseases to which it has been thought that cattle plague is most closely related are, typhoïd or enteric fever, typhus, influenza, dysentery, erysipelas, scarlatina, diphtheria, and smallpox. Let us consider very briefly the points of resemblance and difference. 1. *Enteric fever.*—That there are general superficial resemblances to this human malady cannot be denied, and they are sufficiently obvious to have led a considerable number of German veterinarians to regard the cattle plague as its pathological equivalent or counterpart. Even Professor John Gamgee took this view at the first; but since studying the anatomical lesions more closely, he has abandoned it. The points of difference are, shortly, the absence of the distinctive lesions of Peyer's patches which characterise typhoïd fever, and also the absence of any deposit in the mesenteric glands; on the other hand, the highly contagious nature of cattle plague, the peculiar eruptions upon the skin and in the mouth, the general congestive condition of the mucous membranes generally, and the peculiar lesions of the third and fourth stomachs, find no analogue in the enteric fever of man. 2. *Typhus.*—Cattle plague has been long known in France as the "contagious typhus of horned cattle;" and it will be observed that this synonym has been adopted by M. Bourguignon in the title of his work. But then this writer dwells a good deal upon the nervous phenomena of the disease, which we have seen not to have been remarkable in this country. Except as regards the contagious character, no analogy between the two maladies can be traced. 3. *Influenza.*—It is difficult to see what similarity there is between this disease and cattle plague, except it can be found in the atmospheric conditions which preceded the outbreak; and here the analogy entirely breaks down. There can be no greater mistake, says Dr. Murchison, than to imagine that the epizoötic can result from any atmospheric influence. 4. *Dysentery.*—From this, whatever the similarity in respect of the final bowel symptoms, the contagiousness of the disease and its capability of propagation by inoculation clearly distinguish it. 5. *Erysipelas.*—It is enough to say in regard to this, that there is no known form of idiopathic erysipelas characterised by a similar eruption, and capable of propagation by inoculation. 6. *Scarlatina* is a disease to which cattle plague has been compared by Dr. Smart and others. The points of similarity he relies upon are, the general congestive but non-inflammatory state of the mucous membranes and their epithelial desquamation, the early increase of temperature, period of incubation and critical days, and the presence of albumen in the urine. Some of these points, however, will not bear examination. The essential differences are, the absence of characteristic lesions of the throat, and the peculiarity of the scarlatinous tongue, and of the specific rash and flaky desquamation; the presence of eruptions utterly different from that of scarlatina, and of lesions of the digestive mucous membrane not found in that disease. 7. *Diphtheria.*—Although there are similarities between this disease and cattle plague, especially as regards the exudations on the mucous membrane, the character of the respective exudations differs, and the cutaneous eruption marks an essential distinction between them. In point of fact, the leading features, apart from the fever which must guide us in our endeavour to define the nosological position of cattle plague, are the cutaneous eruption, the eruption on the mouth as it appears at the early stage of the disease, and the lesions of the mucous membranes. Whatever the cattle plague may precisely be, it is no doubt to be ranked among the contagious exanthemata; and to none of these is it more similar in all these respects than it is to *smallpox.* Is it a form of bovine smallpox? if not, is it in any way allied to this disease? These questions have given rise to a great deal of discussion, both in medical and non-medical circles, and the arguments *pro* and *con.* demand our serious attention.

Let us now, then, take up a point in the pathology of the disease which we have purposely deferred until this time—the precise nature of the eruption on the skin, and of the early appearances upon the gums and lips. The observation is common to all the reporters of the Commission, that the morbid process in the mouth and skin is essentially the same, that in both instances it is only the superficial structures which are involved, and that in both the morbid processes are most marked where the glandular structures are most numerous and largest. In the mouth, fauces, and pharynx, the disease essentially consists in congestion of the mucous surface, with increased formation and softening, and tendency to separation of epithelium, with preternatural activity of the mucous glands. The exudation on the mouth and fauces was found, on microscopical examination, to consist of epithelium and nuclear bodies something like pus on the palate. Dr. Bristowe describes "numerous minute orifices (the mouths of mucous glands), from which often beads of transparent mucus stand out, looking at first sight like vesicles;" mixed up with these he found larger orifices, where the epithelium was excavated. His general conclusion is—

> "That the disease there—if not originating necessarily in the glands, or rather in the course of their excretory ducts—is, at all events, remarkably influenced by these organs: that the excoriations commence generally from them as a centre: that probably the disintegration of the epithelium is largely promoted by the action of their secretion. From the circumstance previously referred to—that in other parts of the oral cavity besides the palate the chief seats of disease are those generally in which the mucous follicles are most abundant—it is not improbable that in these parts also the presence of the glands aids in determining the presence of disease, and that the glands play an important part in its subsequent progress." (P. 84.)

Next, as to the skin, there appears no doubt that the sebaceous follicles play the same part that the mucous glands do in the mouth. The earliest change noticed by Dr. Sanderson was the exudation of a semi-solid material on the surface of the skin, on the insides of the thighs, in the immediate neighbourhood of the anus and vulva, and on the chin, of such a character as to render these parts unctuous to the touch. With regard to the eruption, Dr. Bristowe speaks most positively:

> "I have never yet discovered either a true vesicle or a true pustule; nor have I satisfied myself that true pus is ever secreted (excepting accidentally) in this disease by any part of the skin or of the glandular tissues connected therewith. The eruption appears to me to consist in congestion (inflammatory, doubtless) of the capillary plexus of the true skin, and of those involutions of that plexus which are distributed upon the sebaceous glands, with consequent desquamation and increased growth of epidermis, and superabundant formation and discharge of sebaceous matter, and the development in variable proportions of nuclear pus-like corpuscles." (P. 81.)

Speaking of the eruption upon the udder, where it is most characteristic, he says that he has never recognised any actual destruction of surface by ulceration or any other process. Sometimes, when the crust is removed in this situation, the denuded area will be seen to present some roundish-holes; but these are nothing more than the dilated orifices from which the hairs (removed with the crust) emerge, with the ducts of their associated sebaceous glands. When these common orifices have been excessively enlarged,' he has found the sebaceous glands hypertrophied; and in this situation alone has he once or twice recognised what he has believed to be a small accumulation of pus. Again, Dr. Bristowe tells us, as regards the crust, that it mainly consists of sebaceous matter mixed up with fragments of epidermis, and here and there adherent masses of nuclear bodies somewhat resembling pus-corpuscles, but microscopically distinguishable from them. The description of the nature of the eruption as given by Dr. Sanderson does not differ materially from the above. He adds—

> "The alterations of the skin and visible mucous membrane are identical, for in both the corresponding structures undergo similar modifications. That this is so we have direct proof in the often-repeated observation that the morbid process, which commences on the lips, creeps round the corners of the mouth and spreads thence to the cutaneous surface, the aphthous crusts on the former becoming, as has been already stated, continuous with similar patches of softened and thickened epidermis." (P. 15.)

Dr. Beale also says—

> "I have examined many specimens of the eruption in various stages; but I have not seen one elevation to which I should apply the term 'vesicle' or 'pustule.' The central part of the slightly conical projection is softer than the adjacent derm, but it does not contain *fluid;* nor have I ever seen one as much raised from the general surface as the vesicles and pustules familiar to us in the diseases of the human skin." (P. 137.)

Again—

> "In the substance or interior of what would be called the pustule I have found no corpuscles which I could call *pus;* and although germinal matter in the deep layers of the cuticle multiplies and gives rise to many rounded masses which are, no doubt, direct descendants from the deep cells of the cuticle, these bear a small proportion to the smaller particles of germinal matter already referred to. I have never been able to obtain actual lymph or pus from the elevation or from any form of the eruption I have met with. The tissue in the centre of the papule is softened, yet by no means fluid, nor so soft as thick viscid mucus." (P. 138.)

We have been thus diffuse in our statement of the observation of three of the reporters, because Dr. Murchison maintains that distinct pustules are sometimes found in the skin in cattle plague, and employs this as an argument in favour of the close alliance of the disease to smallpox. We do not wish to mis-state his views, and shall therefore quote his own words:—

> "The scabs are occasionally found to be mixed up with minute pimples or elevations of the cuticle, which vary in size, and may be softened in their interior into an opaque yellowish fluid, so as to form distinct pustules, the fluid being confined by a membranous elevation of the cuticle without any superimposed scab. I have not yet succeeded in discovering anything like a distinct vesicle with a diaphanous membrane and limpid contents preceding the pustular stage. On detaching the scabs, the corresponding surface of the subjacent cutis is often found to be depressed and superficially ulcerated; and if the scab have separated spontaneously, its site is often marked by a minute pit or cicatrix. . . . The fluid matter found in the pustules and beneath the scabs is made up of epidermal cells more or less modified in form, nuclear bodies, occasionally true pus-corpuscles and granular manner. . . . It is right, however, to add that in many cases I have observed only the scabs, and nothing like the pustules here described." (P. 71.)

The only explanation we can offer of this discrepancy between the observers is, either that such pustules as Dr. Murchison describes are very rare, and so have been overlooked by the three other observers, or else that Dr. Murchison has been deceived by an apparent similarity, and has misinterpreted what he actually saw.

We cannot follow Dr. Murchison so closely as we should wish in his enumeration of the points of similarity between cattle plague and smallpox, because some of the phenomena which he ascribes to the former are at variance with the observations made in this country; and especially we cannot admit as a point of similarity worthy of a high place in our consideration, the *pustular* character of the eruption. The points of similarity we would dwell upon are:—1. That both are febrile, exanthematous diseases; 2. That both are highly contagious; 3. That both can be imparted by inoculation; 4. That there is a *prima facie* resemblance in the eruption; 5. That in very severe cases of smallpox the internal lesions resemble very closely those observed in animals dead of cattle plague.

> "There is the same congestion and change in character of the epithelium, with tendency to excoriation in the mouth and pharynx; the same congestion of the larynx, with formation of pseudo-false membrane, and tendency to the extension of these conditions into the trachea and bronchial tubes; the same tendency to hæmorrhage beneath and into the mucous membrane of the stomach (the homologue of the abomasum); the same tendency to congestion and submucous extravasation in connection with the mucous membrane of the bowels, especially that of the large intestine; the same tendency to similar affection of the mucous membrane of the bladder and of associated parts; the same tendency to hæmorrhage in connection

with the investing and lining membranes of the heart, the same unusual fluidity of the blood." (P. 94.)

On the other hand, there are dissimilarities between the diseases which must not be overlooked:—1. The cutaneous elevations in cattle plague are not umbilicated. 2. They are not pustular in the strict application of the term. 3. The eruption does not observe that equality in development which prevails in smallpox, since in cattle plague at the time of death the eruption is found in every variety of stage; neither is it the most marked feature of the disease, as it is in human smallpox. 4. The sebaceous glands are more affected in cattle plague than in smallpox. 5. The morbid processes which occur in the air-passages in smallpox consist, not in mere congestive and submucous hæmorrhages, but in the development of an eruption which has a certain resemblance to the cutaneous eruption; that is to say, there are discoverable elevations of the epithelium by the exudation of a fluid between it and the mucous membrane. 6. The pleura in smallpox is very apt to present inflammatory lesions, while in cattle plague the serous membranes generally are remarkably exempt from attack. 7. The abdominal lesions described as observed in severe cases of smallpox are, after all, very rare. Dr. Gregory says that "the freedom of the abdominal viscera from urgent symptoms during life, and from all trace of disorganisation after death, is a remarkable feature in the disorder."[1] On the other hand, abdominal lesions are amongst those most commonly met with in cattle plague. Now, it is quite true that many of these points of difference may be accounted for by differences in the organisation of the skin in men and cattle, and possibly by other differences in their physiological characteristics. Let us see, therefore, what another line of inquiry has brought out for our information.

If cattle plague be a bovine equivalent of smallpox, there are certain expectations we should form respecting it. We must ask the indulgence of our readers while we enumerate them, and attempt to discover how far they are met by the facts in our possession; for we feel we are entering upon the discussion of perhaps one of the most difficult problems in pathology, yet one which deserves a deeper investigation than it has yet received—the nature of what may be termed the *allotropy* of specific diseases. The most remarkable example of such a relation between diseases is furnished us by smallpox and vaccinia. If a number of cows be inoculated with the matter of human smallpox, in a limited number of cases the inoculation succeeds, but the disease produced is *not* smallpox; that is to say, it is not smallpox such as we meet with it in the human subject; neither is it cattle plague, nor yet the Mátá of Bengal. The result is an eruption of vesicles presenting the physical characters of cowpox, which, when re-inoculated upon the human subject, does not again produce smallpox but cowpox, and this only at the points of puncture, having lost its capability of producing a general eruption, and also its capability of transmission by ordinary contagion. Nevertheless, persons who have been thus inoculated become insusceptible of the contagion of smallpox. Now it is quite within the range of possibility that there may be more than one such modification of smallpox. It is quite possible that the bovine species may not only be susceptible of the form of smallpox which we know as cowpox, but of some more severe and fatal form, such as in man smallpox is as compared with vaccinia. It is quite possible that cattle plague should be such more fatal form; it is quite possible that even a third such modification may coexist in the Mátá of Bengal. But if such a relation between these diseases exist, we should expect that the fact of having suffered from the one would give an immunity from suffering from the other—that successful vaccination, in short, should give immunity from cattle plague, and an attack of cattle plague render a beast insusceptible of vaccine inoculation. This is matter for experiment, and experiment has been made. Dr. Sanderson gives a detailed account of eleven animals (eight of them sent by the French Imperial Government, having been previously successfully vaccinated), and in all of which vaccination failed most decidedly in either preventing the natural result of contagion or cattle-plague inoculation, or in modifying in any way the course of the disease. Dr. Murchison also relates the history of two cows vaccinated in the country by Mr. Acton, both of which took cattle plague, and died at the Royal Veterinary College; the one having been inoculated by Professor Varnell, and the other having taken the disease by natural contagion. Neither has the trial of vaccination as a preventive in the case of herds of cattle been more successful, nor has the previous passage through natural cowpox given any immunity from cattle plague. And as respects the previous occurrence of cattle plague giving immunity from cowpox, we have the evidence of the Edinburgh Cattle Plague Committee to the effect that several cases had been reported to them in which cattle that had had cattle plague and recovered, had within a fortnight thereafter been attacked with cowpox.

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[1] 'Library of Med.,' vol. i, p. 308.

Further, smallpox is communicable from man to the cow by inoculation, producing the allotropic disease cowpox. Is cattle plague communicable in like manner from the cow to man, producing an allotropic disease in him? With one exception, the whole history of cattle plague here and on the Continent replies to this question in the negative; and Professor Gamgee tells us, in his work (p. 200), that he has inoculated himself more times than he could venture to name with cattle-plague virus, without suffering from any eruption akin to variola or vaccinia; and to show that he was susceptible to the latter, he was subsequently vaccinated with complete success. The exception alluded to is the case of Mr. Hancock, a veterinary surgeon of Uxbridge, who having accidentally pricked the back of his hand during a post-mortem examination of a bullock dead of cattle plague, had found at the punctured spot a vesicle, which Mr. Rayner of Uxbridge, Professor Spooner, Dr. Quain, and Mr. Ceely are said to have recognised as presenting the characters of a vaccine vesicle, according as each saw it in its several stages of advance or recession.[1] But this case is far too exceptional to prove any relation between the two diseases, if any other probable explanation of Mr. Hancock's vesicle can be suggested; and such an explanation might be found in the suggestion that the animal from which the inoculation was received might at the time have been labouring both under cattle plague and cowpox. We do not know that this was not the case; and even if the animal had been free from cowpox, there remains the possibility that the knife which inflicted the wound might have been charged with vaccine virus from some previous carcase. We cannot help calling to mind a similar explanation of the asserted communication of syphilis by vaccine lymph in the instance of some of the Italian cases about which so much was written a few years ago. Besides, if it is true that Mr. Hancock's vesicle was really derived from the cattle-plague virus introduced by the puncture, "we have to contemplate," as one writer has shrewdly put it, "the startling though not impossible conclusion . . . that the smallpox (Rinderpest) virus of the bovine species acts upon the system of man just as the smallpox virus of the human species acts upon the system of the ox."[2]

This subject is so important, that at the risk of wearying our readers we shall endeavour to exhaust it. Dr. Murchison dwells at some length upon the evidence furnished by the Mátá of Bengal, a description of which is readily accessible to British practitioners from the pen of Dr. Macpherson.[3] This disease has been said to resemble the cattle plague in its symptoms, and there is sufficient evidence to show that when inoculated upon the human subject the result is the vaccine vesicle. In fact, lymph from children thus inoculated by Dr. Macpherson was sent all over the country, and became mixed up with vaccine lymph previously in use. The chief difference in the vaccinia thus passed through a number of children in succession was an earlier development of the vesicles, which were more acuminated, sooner becoming filled with pus and accompanied with more fever. From the description given of Mátá by Dr. Macpherson, we cannot regard it as identical with cattle plague; it appears rather to be the same disease as our indigenous cowpox, modified as human diseases, and even cattle plague, are by climate and race. Any argument, then, derived from the production of vaccinia by inoculation with Mátá virus must, we conceive, fall to the ground.

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[1] 'Third Report of Commissioners,' p. 77; and 'Med. Times and Gazette,' 1866, vol. i, p. 48.

[2] 'Med. Times and Gazette,' 1866, vol. i, p. 94.

[3] Ibid., p. 45.

The general conclusion which we think must be arrived at is, that cattle plague is an exanthematous, contagious, febrile disease, approaching in its general resemblance more closely to smallpox than to any human disease of the class that we are acquainted with, yet essentially distinct therefrom and *sui generis*.

VI. Cattle plague commences in any fresh locality, and propagates itself by contagion, and by contagion alone. This is the final result of all experience both on the Continent and in this country, and is the basis of all the preventive measures which have at any time been proved serviceable in checking the progress of the disease. Still, although we may all unite in this opinion now, there were not wanting those who at the first outbreak of the disease in this country disputed it, maintaining that the outbreak here took place spontaneously from some incomprehensible atmospheric condition; that it was generated by the extreme heat of the weather and the drought of the previous year; that it was generated at the Metropolitan Cattle Market; and, lastly, that it was generated in the London cow-sheds. It is worthy of remark, that the persons who in their evidence before the Commission denied the origin of the disease here by importation were persons more or less concerned in the importation and sale of cattle, while the theory of its origin in the unsanitary condition of the London cow-sheds was chiefly maintained by the Chairman of the Markets Committee of the Corporation of the City of London, and the Veterinary Inspector of the Metropolitan Cattle Market, whose business it was to have recognised and reported the first cases that occurred there. It is in evidence that diseased cows to the amount of 200 were seen by Mr. Priestman in the Metropolitan Cattle Market on July 10th, while that on July 1st, cattle were bought at the Metropolitan Cattle Market which carried the disease into

Norfolk; that cattle exposed in the market on the 22nd, 26th, and 29th of June were reshipped to Rotterdam on the 2nd of July, and carried the disease with them into Holland; and that as early as June 19th, cows purchased in the market were attacked soon after with the disease, these being the first cases in the London sheds. And yet, although Professor Simonds called the attention of the Privy Council to the existence of the disease in London on July 10th, the Market Veterinary Inspector asserted that the first case that he saw at the market was on August 6th. The value of evidence as to the origin of the disease from such sources may, after this, be fairly called in question. But other evidence is at hand to prove that the disease could not have arisen, as asserted, in the London cow-sheds out of any combination of unsanitary condition that is conceivable. The London cow-sheds are, for sanitary purposes, placed under the supervision of the Metropolitan Medical Officers of Health; and their testimony in this respect is worthy of all credence, and is remarkably unanimous. The most accurate statement made to the Commissioners on this matter was given by Dr. Ballard, the Health Officer for Islington, where the earliest cases occurred; and he furnished the Commissioner with a table showing the order in which the cow-sheds in his parish were attacked, the habitual cleanliness of each establishment, the size of the sheds and cubical space allowed to each animal, the sources of water supply, mode of disposing of the dung, the number of cases occurring in each shed, &c., together with tables showing the apparent influence exerted by each of these conditions. We quote the general conclusions which he arrives at:—

> "1. The cattle plague in Islington has attacked the larger establishment, in preference to the smaller.
>
> "2. There is no evidence of the origin of the disease in mere want of cleanliness.
>
> "3. The crowding of cows within sheds appears to have favoured the outbreak of the disease in some instances.
>
> "4. Those establishments which supplied their cows with water from superficial wells suffered more than those which supplied them with water furnished by the New River Company (filtered).
>
> "5. The emanations from dung stored within the shed appear, in certain instances, to have favoured the outbreak of the disease. The general capaciousness of the shed, or the reverse, appears to have exercised no remarkable influence over the general result of the infringement of a sanitary law; but the influence of crowding of the cows seems to have predominated over it where the crowding was excessive.
>
> "6. The smallest establishments, from the less liability to any intro-

> duction of contagion, exhibit more distinctly than the larger establishments the baneful predisposing influences of crowding, and the emanations from retained and fermenting dung stored within the sheds. The influence of a supply of water derived from the superficial wells is most obvious in the establishments of medium size. In the largest establishments the influence of all these predisposing causes seems to have been overridden by some more important and immediate cause, such as would be the very much greater liability to the introduction of the specific poison from without."—*Second Report of Commissioners*, p. 70.

In point of fact, the establishments first attacked in Islington were the cleanest, most commodious, and best managed in the district; while some of the dirtiest, closest, and worst managed were not attacked until a late period. This evidence is not unconfirmed. Dr. Whitmore, the health-officer of St. Marylebone, says—

> "My experience is that the disease in Marylebone has raged most in those sheds where the greatest attention to ventilation, cleaning, and sanitary arrangements generally had been observed. It is a curious fact, that in some of the largest and best ventilated sheds in the parish, the disease has been very prevalent; whilst in the small, close, confined sheds, the disease, though it has broken out, has only broken out lately. . . . I find that where there have been the largest number of cows, the disease seems to have been more rapidly propagated—if I may so speak—than where there were a smaller number."

He adds, that in those small sheds that escaped no new cows had been introduced from the market. Similar evidence is given by Dr. Hillier, the health-officer of St. Pancras; and Professor Simonds stated that the disease broke out only at a late period, among a lot of cows kept under most unsanitary conditions in a cellar in Warwick Lane. And while such evidence disposes at once of the theory that the London outbreak originated in the filthy condition of the London sheds (which, by the way, all agree to be in a vastly improved condition as to sanitary arrangements than prior to the legislation upon them a few years ago), it points most decidedly to the influence of conditions favouring the introduction and diffusion of a contagious cause.

What, then, is the influence exerted by bad hygienic conditions in the case of cattle plague? Precisely the same as is exerted in the case of scarlet fever, or any other similar specific human disease: none at all, unless the specific contagion be somehow introduced; and then they act each in its own way, either intensifying the operation of the poison, or promoting the communication of disease from one individual to another.

It is upon contagion, then, the modes in which it may be conveyed, and the circumstances which modify its influence, that we must concentrate our attention in studying the etiology of cattle plague. Let us begin by studying the results of experiment. It has already been stated that the specific contagion can be introduced into the system by inoculation; how long will the virus maintain its activity? We know that under favorable circumstances vaccine virus may be kept for a very long period in a fresh or even dried condition. With respect to cattle-plague virus, a great many experiments have been made.

> "It had been asserted by the directors of the Inoculation Establishment (Cherson) that cattle-plague virus became mitigated by being kept without losing its virtue; so that in animals inoculated with such virus, although the symptoms which resulted were of the most trifling description, consisting mainly of lachrymation, anorexia, and cough, the inoculated animals were protected from future attacks. For the purpose of ascertaining in how far this assertion was well founded, twenty animals which had been inoculated (Bondarewka) with lymph which had been kept for six or seven months were subjected to experiment, six being inoculated with fresh lymph, the remainder exposed to natural infection. The results showed that the animals were not protected from the disease; ten of them became ill, and five died.
>
> "With reference to the question of the activity of the kept virus, Professor Roschnow inoculated animals with various specimens of virus preserved in capillary tubes for periods varying from one month to eleven months. Fourteen animals were inoculated without result in any case.
>
> "Professor Meterberger appends to this notice a statement that other experiments were made in the same year by Professor Rawitsch, in conjunction with Professor Jessen. Rawitsch arrived at the following results, with which, however, his coadjutor did not concur:—1. Inoculation with old virus was ineffectual. 2. Animals inoculated with old virus subsequently contracted the disease when reinoculated or exposed to natural infection. 3. Inoculation with fresh virus was attended in the institution with very considerable mortality; but in a neighbouring village, where an epizoötic of cattle plague had already prevailed for some time, eighty animals were inoculated, of which only sixteen died. Four cattle which recovered from disease after inoculation could not again be infected by any exposure."—*Third Report of Commissioners*, p. 24.

Sergegew also preserved lymph in capillary tubes with both ends hermetically sealed for nine months and nineteen days, and when it was tried upon two calves it remained without effect.[1]

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[1] Gamgee on 'Cattle Plague,' p. 192.

M. Raupach, however, found in the course of his inoculation on the estate of the Grand Duchess Helena Paulowna, at Karlowka; that virus preserved in ice from the 10th or 11th of June to the 27th of July was capable of communicating the disease. One batch of successful inoculations was made with virus fifteen days old. Next, as to the results of ordinary observation. This subject—the modes in which the contagion could be communicated and destroyed—was fully discussed at the two International Veterinary Congresses, held respectively at Hamburg in 1863, and at Vienna in 1865. A general opinion was expressed, that the contagion of cattle plague was readily destroyed by free exposure to the air. In proof of this fact, Dr. Rawitsch made the following statement:—

> "On the 20th of June, 1863, Professor Jessen and myself obtained at Sars Koge-Selo virus from animals which were very sick. On the 4th of July we arrived at Orenburg, and a certain number of cattle were inoculated with this matter without producing any results. The same cattle were at a later period inoculated with fresh virus; and then we had sufficient opportunity to learn the pathological anatomy of the disease. Many such cases occurred. Once we obtained some virus in a village; a few hours later we inoculated with it, without producing any result. I now come to the subject under notice. Many experiments were made with dried skins, and with hides which had been hung up in the open air and exposed during twenty-four hours. These skins never produced a case of infection. On the same cattle were afterwards laid fresh hides, or they were inoculated with fresh virus, and they died."—*Gamgee*, p. 614.

Dr. Fuchs confirmed this opinion, and the result of the discussion was, that perfectly dried skins and wool, which is always necessarily exposed to the air for some days before being packed, might be admitted into any country fearlessly; and for the same reason, apparently, no disinfection was considered necessary in the case of cow-hair and pigs' bristles. Some further experiments upon this subject were made in 1853 in Russia, where the skin of an animal which had died of the disease was divided into halves: the one half, in a raw condition, was placed upon a healthy beast, which immediately caught the contagion; while the remaining half, after exposure in a Russian vapour-bath to a temperature of 40° R., was placed on an animal which nevertheless remained in good health. Professor Jessen challenged the mention of one single instance in which the disease had been spread beyond the Russian frontier by means of dried hides.

That the morbid discharges from sick beasts and fresh hides may convey the disease, there is thus no manner of doubt. It may also be communicated by the manure. Professor Simonds states, that he has no doubt that it was partly thus spread in this country by the manure from cow-sheds, knackers' yards, and from the Metropolitan Cattle Market. During the earlier months of the epizoötic, we were much struck by the negligence exhibited in this respect at the knackers' yards, where large numbers of dead cows were being disposed of, the manure being taken away by the contractors as if there were nothing at all peculiar in it, for distribution among farmers in the country. Dr. Hertwig stated at the first Congress, that dung was known to have transmitted infection even after it had lain in a frozen state during four weeks; and Dr. Haubner, that the dung of animals suffering from the disease, notwithstanding that it had been brought out in autumn and been frozen throughout the winter, yet communicated the contagion on the following spring. Neither is there any doubt that hay and other fodder brought from premises where the disease had occurred will also convey the contagion. Haubner related an instance in which hay which had been kept in a loft situated over infected stables, and had been used as fodder after a lapse of four months, had communicated the infection; and we ourselves noted a remarkable instance in which a gentleman, in the suburbs of London, who kept two cows for the use of his family, remained exempt until a few days after receiving some brewer's grains from a dairyman who had been accustomed to supply him occasionally, but in whose shed at this time cattle plague had broken out. A great deal has been said about the intense contagiousness of this disease, and about the readiness with which it may be conveyed by persons visiting infected sheds and subsequently unaffected ones, but we are disposed to think that there has been a little exaggeration in this respect. So long as no portion of the discharges come upon the hands or person of the visitant, and he refrains from coming into close relation with healthy beasts, it is doubtful if any contagion is likely to be conveyed. Dr. Ballard informed the Commissioners, that although he had at the early part of the epizoötic two veterinary inspectors constantly employed in visiting the cow-sheds in Islington, there was no evidence whatever that they had in any instance conveyed the disease. The only precautions they adopted were, to avoid handling the diseased animals, to avoid visiting healthy sheds after having been in an infected shed, and taking care to wipe their feet as clean as possible on leaving the latter. Whatever of the contagion they chanced to carry away upon their clothing was probably rendered innocuous by the subsequent free exposure to the air. On the other hand, where such simple precautions are not taken, the person of a visitant may be the medium of conveying the contagion. Dr. Whitmore, of Marylebone, stated that he was afraid he was a personal illustration of this, since, at the outbreak in that parish, he made it a special business to investigate the disease, and went from shed to shed, and unfortunately from beast to beast, and examined them carefully for that purpose, but found that the disease followed in his track. In those several sheds which he had visited, five or six days after he had been to them, and where he had reason to believe no one else had been, the disease broke out.

It is no sufficient argument against the invariable origin of the disease in contagion, to say that the medium of contagion cannot always be traced; nor yet that herds have remained intact, although no special precautions have been taken, in the midst of a district where infected cattle surrounded them on all sides; nor yet to argue from the impossibility of tracing the introduction of the cattle plague into the country from some one particular importation. The difficulty in tracing the medium of contagion is no greater than is met with constantly in medical practice; yet few people at the present day doubt that every case of scarlatina and smallpox that they meet with has had its origin in some particle of contagium, and do not argue from their ignorance of the medium that the disease in such instances must have had spontaneous development. It is very possible that flies settling upon the manure, or discharges of diseased beasts, may sometimes be concerned in distributing the contagion; and there is some evidence in favour of the view, that the wind may occasionally, and under favorable circumstances, be a means of carrying it to considerable distances. This is supposed to be the explanation of a renewed outbreak in Aberdeenshire, after a complete lull in the epizootic from the 28th December, 1865, when the last diseased beast was slaughtered, until the following 19th of January. On that day it appeared simultaneously in two new centres, on the 23rd at a third, and on the 1st of February at a fourth, each of which was at least seven miles distant from any of the others, and from any previous seat of disease. Most careful measures had been taken in the county to destroy all contagion, and to prevent its importation. No food other than the produce of the farm (oil-cake excepted) had been used, and there was no ground for suspicion that the disease had been brought by persons or articles of any description, except in one case, where some pack-sheeting enclosing a piece of beef, and which had done similar service probably in London, and was then used unwashed by the milk-maid as an apron, had been received by the farmer from Aberdeen at Christmas. The reporter, however, states that he is by no means satisfied of the accuracy of this story of the pack-sheeting. We think it best to state the view of the Association in the words of their own report:—

"Without asserting that it is altogether impossible that disease could have been brought to all these places by persons, animals, or articles, the amount of negative evidence is so strong that we are certainly justified in looking for some other means by which the disease may have been communicated. Referring to a map of the counties of Aberdeen and Kincardine, it will be seen that the five new centres of disease lie nearly in a straight line from S.W. to N.E.; and further, that if this line is continued south-westwards, it passes through the parish of Fettercairn, distant on the map some ten miles from the nearest of the five cases, and forty from the most distant. Since the 1st of January about sixty farms in the parish of Fettercairn and immediate neighbourhood have been attacked by the disease; and as the farmers have attempted to cure, that district necessarily became a hotbed of rinderpest. Can there be any connection between this hotbed of disease and these five outbreaks occurring at intervals in nearly a straight line running N.E. from it?

"I have before me meteorological observations for the month of January, including the ordinary particulars of wind, rain, temperature, &c., registered by Mr. Beverley, of the Grammar School of this city.

"Endeavouring to trace a connection between the atmospheric conditions and these five outbreaks, I assume a period of incubation of about five days, which is the experience in this county.

"Cases No. 1 and 2, twenty-five miles apart, occurred on the same day, 19th January; and assuming the five days of incubation, we must look to the weather of the 13th or 14th as the days of contagion.

"On the 13th and 14th, the wind is recorded as exceptionally high, from the S.W. (being the tail of the great storm of the 11th and 12th in the South of England, which only then reached us). On the 14th, the temperature reached a maximum of 51°, being 15° above the average January temperature, and 10° above the maximum temperature of any preceding day of that month. Both days were dry, the rain amounting only to a few drops.

"Cases 3 and 5, twenty-five miles apart, appeared on the 23rd, making, on our assumption, the 18th the day of contagion. On the 17th, 18th, 19th, the wind is recorded S.W., and above the mean pressure for the month, and also higher than on the two preceding and two succeeding days. The temperature of that day reached precisely the same degree as on the 14th, viz., 51°—15° above the January mean. Between the 14th and 18th, the temperature had been considerably lower. On the 18th there was no rain.

"Case 4th occurred on the 1st February, making the 27th January the day of contagion. On the 27th and 28th, the wind was again S.W. On 27th, about noon, and on 28th, considerably above mean pressure. As to temperature, the maximum of 53° (the maximum for the month) was reached on the 26th. On the 27th, the maximum temperature was 51°, the same as in the two previous cases. Between the 18th and 26th, the temperature was considerably lower. There was no rain from the 24th to the 30th.

"From these records it appears that on the fifth day previous to each of these three days on which disease became evident—(1), the wind blew hard from the direction of Fettercairn in the line of these five cases; (2), the three highest temperatures for the month (51°) were reached on these days respectively, except in the last, when 53° was reached a day previously; that in two cases the rain was barely perceptible, and in the third there was no rain for some days.

"If, then, there is any connection between the disease in Fettercairn and the outbreaks in this county, the probability appears to us to be, that on those days of high winds and high temperatures, without rain, minute particles of infectious matter were carried by the wind from the district of Fettercairn, scattered in the line of the wind, and took effect wherever they unfortunately found a susceptible subject, which in every one of the five cases was a cow, or heifer in calf.

"The supposition may at first sight appear extravagant, but it is not inconsistent with various ascertained facts. Sand and ashes have been deposited on the decks of ships when 100 miles from land; and last week it was stated that a deposit of salt was found on the windows of the carriages and on the polished work of the engines of a train on the Highland line when crossing the summit level near Struan (the elevation being from 1000 to 1500 feet) during the tremendous gale from the westward, the nearest arm of the sea being about thirty miles, and the Western Ocean sixty miles distant."—*Report of the Aberdeenshire Rinderpest Association.*

If the view of the Association be correct, the distance to which the contagium, in dust, particles of manure, &c., may be conveyed by atmospheric movement must be greater than is generally regarded as probable. Yet there appears to be some reason to believe that the explanation given of this renewed outbreak is the true one.

We must say one word about the asserted introduction of the disease into England by the celebrated Revel cargo. No one can read the evidence given upon this subject before the Commissioners without observing how utterly valueless it is. Contradiction is piled upon contradiction in respect to matters of fact, the truth of which ought to have been readily come at. The principal witnesses, formerly engaged together in business, had quarrelled, and each showed a disposition to give that sort of evidence which would be most damaging to the veracity of the other. All that can be derived from it is, that this was the first cargo of beasts that had been imported direct from Russian territory; but it arrived too long a time before the outbreak of cattle plague took place in this country to be regarded as the cause of the disease. Besides, it is to be kept in mind that communication with the South of Russia is now a matter of a few days only; and in a letter from Mr. Schrader, a veterinary surgeon employed by the Hamburg Government to Her Majesty's Consul-General there, he states that in the course of the spring a considerable number of Hungarian cattle were conveyed from Vienna into Holland through Germany; that at the same time rinderpest had broken out in the neighbourhood of Vienna, and that in the month of May a number of cattle at or near Utrecht, in Holland, had been attacked with it. It was thus quite possible that it passed from the Dutch ports into England so early as the month of May. Hungarian and Galician cattle undoubtedly came in considerable numbers to the English market. One dealer says, that large quantities are sent every week. Hungary and Galicia, from their neighbourhood to the Steppe country of Russia in Europe, are often attacked by the plague, and Hungary at least suffered severely from it last year. The inspection of imported cattle at our own ports, so far as the evidence given by the inspectors goes, is sometimes a very superficial affair. Any beast apparently well seems to have been passed at Hull, where the Revel cargo was landed, as is evidenced by the fact that that cargo of 321 beasts was examined in the space of three hours and a half. There is sufficient proof that beasts in an early stage of the disease may yet retain a great deal of vigour, and a remarkable instance of this is given by M. Bourguignon (p. 170). But, after all, it is a matter of mere curiosity, and not at all important in a scientific point of view, to trace precisely the mode in which the disease was introduced here. We know it must have been imported in some way; it was not here prior to May or June, and it could not have been generated here spontaneously.

It appears that the period of incubation of the disease when taken by natural contagion is somewhat longer than when the contagium is inoculated. Accurate observations appear to show, that when a healthy beast is placed in the same stall or in close proximity with a diseased beast, the initial symptom (rise of animal temperature) is observed on the fourth or fifth day from the first exposure. The first ordinary characteristic symptoms marking what is popularly regarded as the commencement of the disease, would thus occur about the sixth or seventh day. This period corresponds closely enough with the results of general experience. The incubation may be apparently somewhat longer in some instances, and we can well understand how this may arise out of variations in the precise manner in which the contagion is conveyed. Professor Gamgee's experience is to the effect that the incubation rarely extends beyond the eighth day. It has been stated that Steppe cattle exhibit an incubation of the disease considerably longer than other breeds; so long, indeed, that it is an opinion with some, as with Professor Rawitsch, that in these the disease may break out spontaneously in the course of a journey, and when the system is weakened by the privations undergone. This is the explanation he offers of the fact, that a herd may leave the Steppes perfectly healthy, and after three months from its arrival in the North of Russia the disease may appear, and this in instances where the cattle have not passed through stables where rinderpest was prevalent. That the disease may appear to break out on the road during a long journey, may be explained by the fact, that as the disease is ordinarily of a mild form in Steppe cattle, it is quite possible that the earlier symptoms might readily have been overlooked: in addition to which, it is difficult to exclude from consideration the occurrence of chance infection on the road, especially when we keep in mind the long period during which the contagion may lurk in dung, &c. Professor Jessen, whose experience of Steppe cattle is as large as any man's, says that he has not observed a commencement of the disease later than the ninth day. Professor Simonds also gives, in his evidence to the Commissioners, an accurate observation of the period of incubation among ten steppe cattle in the villages Zabezez and Kamienica, in Galicia (First Report, p. 2), and among these the apparent incubation was not prolonged beyond nine or ten days. The early stages of the disease may be readily overlooked in Steppe cattle by a superficial observer. For the reasons stated generally in our previous remarks, we are not disposed to consider it proved either that the disease ever breaks out *spontaneously* in Steppe cattle, or that they "carry the germs" of the disease with them for an indefinite period of time.

It has been determined, as we have seen, that the blood of an infected beast is so far contaminated within forty-eight hours as to be capable of infecting a healthy beast by inoculation. How early in the disease a beast may communicate it by natural contagion is another affair. Probably this cannot occur until some morbid products are given off and discharged externally by the mucous membranes. This harmless period would therefore extend to a period of about four or five days. One accurate observation is recorded by Mr. Smith, the consulting veterinary surgeon of the Norfolk Cattle Plague Association, in which two bulls in whom the disease was at the time incubating were sent to a healthy lot of cows, remaining with them for about twelve hours. The cows remained healthy, but two or three days thereafter the bulls were attacked with the obvious symptoms of the cattle plague, and died from it (First Report, p. 48). At what period after recovery convalescent cattle may be safely herded with healthy ones is undetermined.

VII. One of the most interesting portions of the last report of the Commissioners is the contribution of Dr. Angus Smith and Mr. Crookes upon the subject of "Disinfection and Disinfectants." The subject is so important and embraces so many collateral inquiries, that it cannot fairly be discussed at the tail of an article such as this, which has already swollen out to abnormal dimensions. Starting with the assumption, that the contagium, or "virus," is something not only organic, but organised and living—living in the sense that it is endowed with the faculty of "living as a seed or as vaccine matter may be living, inasmuch as it still possesses reproductive vitality;" that, like yeast, whose cells multiply by feeding upon sugar, excreting alcohol and carbonic acid, so virus-cells feed upon some element in the blood, "whilst at the same time they excrete a poison to which the symptoms of the disease may be immediately due;" and that possibly they may be capable of living and multiplying in other warm liquids beside the blood, and that it is not an unreasonable supposition "that the presence of decaying organic matter or the gaseous emanations from putrefying dunghills preserves, or may even revive, the expiring vitality of germs brought by men, dogs, birds, vermin, or perhaps the wind;"—the first question to be solved would naturally be, whether it is possible to find any chemical disinfectant or antiseptic which will readily destroy it, and at the same time not prove injurious to the animal to be preserved. What is wanted, is a substance which may be applied in a liquid form and yet is volatile:—

> "Which, after first acting on the excreta—the floors, walls, and stalls of the shed—will, by its quality of gaseous diffusion, rise into the air, enter the lungs of the animal, pervade the whole building, and attack the hidden germs of infection, which otherwise would escape. In addition to this, the agent must do its work with as little inconvenience as possible to the cattle and their attendants."

Practically, partly on the ground of expense and partly on the ground of other inconveniences, the choice is limited to the oxidizing disinfectants, chlorine and ozone, aud the antiseptics or "colytics," as Dr. Angus Smith calls them, sulphurous acid and those tar acids which pass together commercially under the designation of carbolic acid. Mr. Crookes states that he started in his inquiries with a strong bias in favour of chlorine and ozone; but the irresistible force of the arguments derived from his experiments has caused him to alter his opinion, and he concludes in favour of sulphurous and carbolic acids. The objection to the oxidizing disinfectants partly lies in the fact that both chlorine and ozone waste their powers in destroying the valuable constituents of manure before they can act upon the septic germs; " that those vapours having strong and fœtid odours . . . . are the first to go, whilst the actual virus of the disease—the organised particles which have no odour whatever—are the last to be attacked." Besides this, chloride of calcium, which would result from the use of chlorine, is very deliquescent, and leaves the woodwork permanently damp; and phosphorus, which would be naturally employed for the development of ozone, is dangerous in inexperienced hands. A series of experiments with carbolic acid, which are fully detailed in Mr. Crookes's paper, tend to show—1. That the tar-acids have no special power of retarding oxidation. 2. That they have scarcely any action on fœtid gases, but attack the cause which produces them, and at the same time put the organic matter in such a state that it never re-acquires its tendency to putrefy. 3. That they do not owe their special action to their coagulating powers on albumen. 4. That they have no special action on purely chemical ferments, such as diastase or synaptase, but that they have a special action on fermentation induced by organised matter, such as yeast. 5. That a solution of 1 per cent. of carbolic acid in water destroys cheese-mites, small fish, and the infusoria which are almost invariable accompaniments of putrefactive fermentation; and that the vapour destroys the life of such insects as caterpillars, beetles, fleas, moths, and gnats. Indeed,

> "The powerful action which carbolic acid exerts on the phenomena of life is the most remarkable property which it possesses. It may be looked upon as the test proper for distinguishing vital from purely physical phenomena; and in most cases its action is characterised by the certainty and definitiveness of a chemical reagent. In the presence of carbolic acid the development of embryotic life is impossible, and before its powerful influence all minute forms of animal life must inevitably perish."—*Third Report of Commissioners*, p. 193.

But, after all, the test of the value of a disinfectant, as of any ordinary remedy proposed for the cure of disease, must lie in its practical usefulness; and Mr. Crookes supplies us with instances in which his method of disinfection has been applied in various farms, whether to protect them from the march of the closely investing plague, to stop the plague after it has invaded a farm, or to protect an animal placed by the side of a diseased beast or in an infected shed; and he has also endeavoured to ascertain how far it is capable of destroying the virus after its reception into the system. On the whole, we must confess that the results of the combined influence of cleansing, fumigation with sulphurous acid and the use of carbolic acid are highly encouraging. The subject is one which has a broader application than the mere arrest of an epizoötic like cattle plague, and we earnestly commend the study of the papers by Dr. A. Smith and Mr. Crookes, and more especially the consideration of the practical observations of the latter, to all our readers.

When once cattle plague has spread to any extent in a country, the only proceedings which can be adopted to stay its progress are isolation of healthy herds and disinfection. Cattle are safer in well-cleansed and thoroughly disinfected sheds than they are in the open fields. "Stamping out" by slaughtering infected beasts and herds is no doubt the proper proceeding when the disease is limited, and all experience points to its efficacy under such circumstances, but it is clearly not applicable when cattle plague has spread extensively. The practice of inoculation with cattle-plague virus has its advocates on the Continent, and has been especially recommended for use in the Russian Steppes, in what may be regarded as the home of the disease; but apart from the objection to its application elsewhere, which is familiar to us in regard to the analogous operation as a prophylactic measure in human smallpox, there does not appear to be any method by which the energy of the virus can be reduced, and at the same time a power of resistance to ordinary contagion be imparted.

Quarantine, as preventive of the introduction of cattle plague into this country at any future time, would be almost an impossibility; so that a great part of the discussion upon the subject at the Vienna Congress has an interest for us only so far as the facts are concerned which formed the basis of its recommendations. Professor Hertwig and others showed clearly that where enforced with the greatest strictness on the frontiers of Continental States, it did not afford an unconditional protection: the stricter the quarantine, the more smuggling was carried on; and the greatest protection against the spread of the disease consisted in the veterinary police regulations employed in the interior on the first occurrence of any outbreak.

With respect to the treatment of plague-stricken cattle, it can scarcely be said that the experience of this country has been altogether fruitless. It is true that when the period of incubation is over, and the disease has commenced to show itself by symptoms, the administration of drugs of any kind seems to be of little or no use. This we might almost have anticipated. Perhaps there is a shadow of an exception in the case of carbolic acid, which in several cases Mr. Crookes injected in a diluted state into the veins. There certainly was improvement temporary or permanent, but the results were not decisive; still they are such as to encourage further experiments in the earliest recognisable stage of the malady. But although drugs may be of no service, the dieting of the patient does appear to be a very important matter indeed. The experience of the Edinburgh Cattle Plague Committee is to the effect that recovery was promoted by the exercise of great care in supplying the beasts with such soft and digestible food as was calculated to afford sufficient nourishment without damaging and irritating the diseased membranes with which it must come into contact. The following statistics are furnished in proof of this point:—In the instances of 95 cottagers' cattle which were generally fed on mashed food, 73·7 per cent. of the cases recovered; of 105 beasts out of larger stocks where dry food was often given during convalescence, 57·5 per cent. recovered; of 303 beasts treated with mixed food of mashes and hay, 22·2 per cent. recovered; while of 310 cattle fed with dry food and treated medically with drugs, only 13·5 per cent. recovered. Again, dividing the Edinburgh cattle into two groups—one of 303 cattle where the stocks were large (30 to 80 cows), and one of 200 cattle where the stocks were small (under 30 cows), the recoveries among the former were 22·2 per cent., and among the latter 62 per cent.

> "In small stocks, fewer beasts are ill at once; hence there is less concentration of the poison. There is less crowding of the sick beasts; and, the supply of labour being always relatively greater in small herds, a sick beast receives much more nursing and careful feeding than in large ones."

Altogether the percentage of recoveries was 38, the recoveries out of 10,000 treated cases returned by the Veterinary department of the Privy Council being 26·256 per cent. The favorable nature of the Scotch returns is attributed by the Commissioners in a great measure to the smallness of the stocks.

We have in this review been compelled to confine ourselves very much to the phenomena and teaching of the cattle plague as it has appeared amongst our own herds, and have endeavoured to select for special consideration those points which seem to have the most direct bearing upon human pathology, medical practice, and hygiène. The disease, when appearing in other ruminants, such as sheep, goats, deer, &c., does not differ materially either in symptoms or in post-mortem appearances from what is noticed in horned cattle. Beyond this, we have only to say that there is no evidence whatever that the consumption of cattle-plague beef has in any case proved injurious to individuals or to communities which have been fed upon it. The discovery of "Rainey's bodies" in the flesh in inordinate numbers naturally led to the suspicion that they might be an early form of some entozoön capable of lodgment and development in the human system; but of this there is no proof, and so far as our present experience goes they are harmless to man. What these bodies are which have been so minutely described by Dr. Beale, and what relation they bear to cattle plague, are points for future science to determine. So far as inquiries have been prosecuted at present, they have been shown to have no peculiar relation to this disease, since they have been discovered in animals killed while in perfect health.

We cannot conclude without a word in commendation of the admirable manner in which the final report of the Cattle Plague Commissioners has been got up. Nothing can be more beautiful and truthlike than the illustrations with which the several reports forming the Appendix abound, executed in chromolithography in the very best style. The microscopic illustrations of Dr. Beale's report were drawn upon the wood by himself, and according to scale; so that not only may the exact dimensions of any object be easily ascertained with the aid of the scales of measurement appended to each plate, but the delineations themselves may be confided in for accuracy and truthfulness, as they could not have been relied on had this part of the work been entrusted to the hands of any professional draughtsman.

Review IV.

Lectures on Mental Diseases. By W. H. O. Sankey, M.D. Lond., Fellow of the Royal College of Physicians; Lecturer on Mental Diseases, University College, London; Proprietor of Sandywell Park Private Asylum; late Medical Superintendent of the Female Department of the Hanwell Asylum.

It is a matter of congratulation that psychological teaching has at length become an "institution" at some of our great metropolitan schools of medicine. University College has appointed one of her most worthy sons to occupy the new chair which she has created. There is a rumour that King's College, fired by a spirit of emulation, has entertained the same project, and will shortly carry it into effect; and at St. George's Hospital, twelve lectures on Insanity are given every winter session by Dr. Blandford. Thus is being supplied a deficiency which has long been greatly needed.

Since the year 1843, when Dr. Conolly delivered a clinical course at Hanwell, we are not aware that any steps have been taken by which students might acquire some practical knowledge of mental diseases. A few occasional pupils at Bethlehem and St. Luke's have kept alive the fact that two insane picture-galleries are still open, upon a trifling fee, for a morning stroll to the professional *dilettanti*. But these two hospitals have failed to furnish any systematic teaching worthy in the one case of its great endowments, and in the other of its able medical staff.

The natural result, therefore, of this state of things has been (we say it with all due respect, and we speak advisedly) an ignorance on the part of general practitioners in respect of lunacy which is hardly to be credited by any one who has not had an opportunity of noticing the fact. This ignorance shows itself not only in a deficient acquaintance with the laws of lunacy as evidenced by medical certificates, but in a want of familiarity with the symptoms of special disease. Of nothing can this be more truly said than of the general paralysis of the insane, or (as Dr. Sankey prefers to term it) of general paresis. We constantly hear of patients being admitted into asylums, far advanced in this most fatal malady, of whom able and well-educated practitioners have certified that their attack is "recent" and "uncomplicated with any form of paralysis or epilepsy."

But what can the verbal pictures of Drs. Bucknill and Daniel Tuke, or of Dr. Sankey, do towards familiarising students with insanity, if they are not accompanied by objective teaching? And how is this objective teaching to be obtained? Fees will not procure admission into county and borough asylums, where, for the most part, the visiting justices regard them as provinces not to be trespassed upon by any outsiders, who might sometimes call in question the wisdom of committees, or smile at magisterial self-importance. This difficulty must be faced by any future professor of psychology at a metropolitan school, unless he is officially connected with Bethlehem or St. Luke's.

Through the courtesy of the Visiting Committee and the physicians to the Sussex County Asylum, Dr. Sankey was enabled to take his pupils from University College to Hayward's Heath, where Dr. Lockhart Robertson instructed them in general asylum management, and brought under their notice the useful and interesting cases at that time under treatment. Dr. Paul, of Camberwell House Asylum, has done a similar good office, and thus smoothed away some of the difficulties of the new professor in Gower Street.

The lectures which Dr. Sankey delivered he now gives to the profession in a published form. While there is much originality in many of his views, and much to be commended in the manner in which he elucidates them, there are things of great importance which he has altogether overlooked, and which are strikingly "conspicuous by their absence." We can understand, for example, how a general practitioner, who perhaps is more frequently brought in contact with that form of insanity which is allied with the puerperal state than any other, would expect to find some help from a lecturer who for years has been connected with the female department of a great asylum. And yet there is no allusion to puerperal mania beyond a mere statement of the general fact that lying-in women are subject to an abnormal something which takes its name from a condition by which the human family is enlarged and multiplied. Dr. Sankey's rigorous exclusiveness in classification has led him into this strange omission, and into others which justify us in thinking that these lectures are more suited for those who have some previous knowledge of the subject, and propose to devote themselves entirely to the specialty of which he treats, than for those to whom a case of insanity will in the nature of things be rare and exceptional. Their value to the profession at large is thus greatly lessened.

And yet we think there is much to be said for the simple and uncomplicated classification which our author recommends. It is beyond a doubt that in general medicine students are constantly embarrassed by the elaborate trifles by which their teachers seek to point out differences of no appreciable value, and which, when discerned, exercise no influence either upon prognosis or treatment. The great error which authors have committed in classifying mental diseases, has been that of coining some distinctive name from some prominent symptom; whereas the prominent symptom is a mere accident arising in the course of the malady, giving us no sort of clue to the general psychological or pathological condition of the patient. Thus, varieties have come to be exalted into species, and the element of simplicity has been causelessly banished from nearly every classification:

> "To separate one species of disease from another, I hold that it is necessary to prove that one has a different origin, a different progress, and a different termination." (P. 57.)

With a view of helping us out of our difficulties, the distinguished physician of an Asylum proposed to us three years ago a "Rational and Practical Classification," embracing twenty-six different species of disease. He must have had in remembrance that passage of old Burton (quoted by Dr. Sankey) in his 'Anatomy of Melancholy,' where he says—"The four-and-twenty letters make no more variety of words in divers languages than melancholy conceits produce diversity of symptoms in several persons." Or he must have recently perused the 'Leçons Orales' of Guislain, where he says (in words also quoted by Dr. Sankey), "Vingt-trois formes de manie, sans compter plusieurs formes composées non indiquées, voilà, me direz-vous peut-être, un bagage symptomalogique passablement lourd pour la mémoire."

Everywhere, and most consistently, Dr. Sankey protests against the useless verbiage of name-inventors in classification, avowing that "their efforts have tended rather to complicate the subject than to render it more easy to comprehend." He even goes so far as to state that, according to his experience, melancholia and mania are simply stages of one disease; that it will be found on investigating the history of every case of primary mania, that the first symptom was depression of spirits, and that there is no abruptness or suddenness about the transformation from one state to the other. This view had been previously advanced by Guislain, Neümann, and Griesinger; but it has never been so dogmatically stated (on his own admission) as by the author of the Lectures under consideration, who supplements his declaration by the proposition, that in secondary attacks (so-called) of mania, the primary disease has never quite subsided if melancholia has not again initiated the seizure.

> "I think the study of certain of these secondary or recurrent cases proves them to be mere gradations in the absoluteness of the recovery from the primary attack; the progress of the morbid phenomena becomes moderated and intensified at irregular periods, but never actually ceases. They are cases of chronic insanity, therefore, and differ only in having a more marked lucid interval between the paroxysms." (P. 109.)

Instead, therefore, of the "Rational and Practical Classification," embracing twenty-six varieties, Dr. Sankey gives us one much more rational and practical, from whose simplicity are evolved only *two* morbid species, viz.—1. Insanity; 2. General Paresis. But mental symptoms occur also as epiphenomena in certain cases of epilepsy, giving us—3. Epileptic Mania. These are the absolute diseases, and together with the imperfect manifestation of mind, as in idiocy, and with decay, as in old age, constitute the sum of all we have to study. With respect to ordinary cases of insanity, the phenomena exhibited in primary attacks are, chiefly—depression, morbid apprehensions, illusions, constituting an attack of melancholia. These phenomena may terminate—1, in health; 2, in death; 3, in mania; 4, in chronic insanity. The melancholia may remain melancholia, which name-mongers will baptize and classify according to the character and form of the morbid apprehensions. Or it may become acute or chronic mania, according to the measure of duration and intensity. These also may draw largely upon the fancies of lovers of nomenclature. Under all the above conditions, when the tendency is neither towards death nor recovery, it is towards imbecility or dementia.

It will be seen from the above classification that our author regards general paresis as a distinct morbid species, not answering to Dr. Skae's definition as "a form of insanity complicated with general paralysis, or as a general paralysis complicated with insanity." In this opinion he is supported by Parchappe, Jules Falret, Delasiauve, Salomon, and others.

We prefer giving Dr. Sankey's arguments for isolating general paresis from diseases of the mind proper in his own words.

"1st. It appears to me that if the paretic symptoms are mere epiphenomena, then the duration of the disease should be longer in general paresis than in insanity generally; but the reverse is the fact.

"2nd. If the motor symptoms are merely, as it were, engrafted upon ordinary cases of insanity, then cases of second and third attack should be equally liable to have the paretic symptoms engrafted upon them as first attacks. But such is not found to be the fact. . . . . . .

"3rd. If the paretic symptoms are epiphenomena, they should occur as frequently among the old inmates of asylums as among the more recent cases. If, indeed, these symptoms are to motility what imbecility is to the intellectual faculties, we ought to find general paresis more frequently developed in the old cases than in the recent; but such is not the case.

"4th. If the disease is identical with other forms of insanity, and the paresis a mere accident, then the frequency of the predisposing causes should be the same in all cases; whereas I found in my paretic cases hereditary predisposition existed in 14½ per cent. instead of 20 per cent. among females, and 17½ per cent. instead of 22 per cent. among males. And in examining into the evidence of hereditary predisposition in paresis, there was found to be evidence of paralytic symptoms in many of those from whom the predisposition came.

"5th. If general paresis is one and the same disease as insanity, then in those instances in which several members of a family are insane it should happen that some should be found with paretic symptoms, and some without. There were fifty-five patients in Hanwell related by blood to other patients also under treatment. There was but one paretic, who was an epileptic, with motor paresis, the niece of whom was also in the asylum.

"Lastly. General paresis, besides the difference which it manifests to insanity generally, in affecting the sexes, the different classes of society, and different localities, to a different degree, also appears to affect individuals of a peculiar temperament. Among the patients affected with it, we find chiefly those who have lived a fast life; reckless, imprudent individuals, who seem from their history to have been swayed through life by their lusts and passions."

Dr. Sankey admits himself that the above arguments are not conclusive; but he thinks that they strongly favour the opinion of the specific nature of the disease as a distinct species rather than a variety of mental alienation. He thinks also that sexual indulgence is one of the chief factors in producing this hopeless malady, and that the predisposition to it of the different classes of society is, in the order of sequence, that also of the subjugation of the animal passions, viz.—1, males of the lower classes; 2, males of the upper classes; 3, females of the lower classes; 4, females of the upper classes. Among the latter, Dr. Conolly distinctly states that in the whole of his large experience he never met with a single case.

Our author graphically describes the three stages of general paresis, and the various modes of its invasion. In alluding to the gradual loss of excito-motory power, he confirms the opinion of Dr. Bucknill as to the effect of tickling the soles of the feet: "In ordinary paralysis there is no loss of excito-motory sensibility, while in general paralysis there is a great loss of this power." We are bound to state that our own experience does not enable us to endorse unequivocally the statements of these two psychologists; and we may add, that the opinion of MM. Brierre de Boismont and Duchenne de Boulogne upon this subject is directly at variance with that of their two English *confrères.* With respect to the morbid anatomy of general paresis, Dr. Sankey agrees with Wedl and Rokitansky in thinking that there is in the cerebral substance an overgrowth of connective tissue on the outer walls of the little arteries and veins, but that this pathological condition is by no means confined to this specific disease. He does not think we are yet in a position to demonstrate any distinct alteration in the nerve-elements or nervous structure of the brain, but inclines to believe that a morbid condition of the capillaries of the cortical substance is the real explanation of general paresis.

While the amount of alteration bears no relation to the date of disease, degree of imbecility, or impaired motility, this abnormal appearance exists but very rarely in any other form of mental disease. Rokitansky and Wedl, however, dissent from this view. Dr. Sankey thus expresses himself upon this point:

> "The capillaries enter the cortical substance of the brain from the under surface of the pia mater in the direction of a right angle to the circumference. There appear to be two kinds of them, one of which is short, and divides into a tuft of minute capillaries close to the peripheral surface of the brain; and the other is larger and pierces deeper, and then divides in the same manner. The trunk of the larger artery especially is found at places curiously twisted in cases of general paresis; and I have found some disposition to twisting or varicosity in every case of the disease I have hitherto examined. All degrees of tortuosity are to be found in different brains, and great variety in degree in one brain—from the absence of straightness to the most complicated knots. My own examinations of the capillaries in about twenty cases of insanity, and seven of which were from patients who died of general paresis, lead me to the conclusion that the capillaries of the cortical substance are more or less diseased in every case of general paresis." (Pp. 174, 175.)

We have dwelt thus upon general paresis, because it appears to us that Dr. Sankey has handled the subject in an able manner, and because of its growing importance, by reason of increasing frequency.

In treating of the general predisposing and exciting causes of insanity, Dr. Sankey does not fail to point out the many disturbing influences which lessen the value of statistical conclusions, and should render us sceptical about all dogmatic teaching based upon formulated tables. It is difficult to isolate the factors of disease, and determine the relative measure of physical and moral causation. Under the large and general term of "civilisation" the material and immaterial influences are happily blended; and in spite of much which has been said and written to the contrary, we are driven to the inevitable conclusion "that the wealthier, and therefore surely the more educated and higher civilised class, is less liable to insanity than the middle and lower class."

Wherever men are grouped together in large masses—as where civilisation marches—there are grouped also the social vices as well as the social virtues. Ignorance, dirt, intemperance, promiscuous intermarrying, sexual excesses, and sensuality of every kind, are (as Dr. Sankey happily expresses it) the "camp-followers" of intellectual progress. They go where civilisation goes, and there is no staying their influence; and the result is, that the proportion of pauper insane to pauper sane is as

1 to 50, while the proportion of non-pauper insane to non-pauper sane is as 1 to 3982.

We cannot bestow the highest praise upon the style in which these lectures are written. There are many obscure and many slipshod sentences which are quite unworthy of an erudite physician. The first lecture might well have startled his class, for Dr. Sankey presents to them a sketch of mental science which it is by no means easy to understand. Nor does he give them any very cheering accounts of therapeutic agents in the treatment of disease. He has little or no faith in opium, or digitalis, or tartar emetic, or Indian hemp. Mustard baths and shower baths seem to him to be the best sedatives. But the entire subject of therapeutical treatment is dismissed in a page and a half; whereas he has devoted thirty-seven pages to the moral agency by which insanity may be controlled and regulated. Its legal relations are summed up in a very useful chapter, which may be read with profit by every practitioner.

If Dr. Sankey has failed to produce a course of lectures equal in their completeness to his known attainments, he has at least given us evidence of much painstaking and research.

Review V.

The Harveian Oration, 1865. By Henry W. Acland, M.D., F.R.S., Regius Professor of Medicine in the University of Oxford. Macmillan & Co. 1865.

The Harveian Oration was last year, for the first time, pronounced in English. Harvey, were he alive, would probably not disapprove the innovation. So keen and forward-reaching a spirit would have fully appreciated the convenience and the freedom which the natural language of use and common life gives for the adequate discussion of the discoveries and subtle ideas ever newly emerging, as our knowledge of nature is enlarged, deepened, and corrected. Yet, perhaps, some not altogether groundless regrets might have passed through his mind at seeing, in one instance after another, the disuse of that powerful and accurate instrument of expression which he employed, and which in the hands of those who know how to use it, as many did in his day, is no contemptible aid for securing precision and directness of speech, even in scientific subjects, while it is almost incomparable in clothing memorable thoughts in memorable forms, and in impressing the attention by the force which is given by terseness, apt felicity, and the majesty of severe and well-weighed statement. To be able to write in Latin is not only an accomplishment, but a real power, the fruit of exercise, and involving the cultivation of very important faculties. We write so much in English, and Latin is such a discipline against the dangers to which English writing in an age like ours is liable—the dangers of looseness, vagueness, and generally of slovenliness in fitting together thought and word, that we look with some jealousy on the encroachment of English in those high places of academic form and state where Latin has maintained its ground, and there were inducements still held out to cultivate its use. A dead language, even though a universal one, would be an intolerable yoke, which it is well to have thrown off, in the actual work of modern scientific life; but as long as good, clear, scholarlike writing is of value, there are reasons for encouraging the practice of composition in Latin; and nowhere does it seem more becoming and more in place than in a commemorative solemnity which gathers together scholars. In the case of the Harveian Oration the advantages of the change are obvious; yet it ought to be remembered that there has been some loss, too, and that it will not be compensated except by increased intrinsic value and interest in the new series of English discourses.

Dr. Acland opens it with an essay which, in its English form, has not departed from the traditions of literary refinement and elegance befitting the occasion, while it enters on a really critical consideration of an important question of scientific philosophy. It is one of those questions which are as old as philosophy itself, and which probably will continue as long as philosophy continues to interpret the facts of nature and the forms under which the mind views them; but it is one to which the amazing development of physical knowledge in recent years, and the apparent tendencies of philosophical speculation which have accompanied it, give great prominence. In commemorating Harvey, Dr. Acland is led to contrast one of the leading philosophical ideas on which Harvey worked, and which avowedly guided him in that method of research which has made his name illustrious, with the opposite conclusions on the same point drawn in our day by some of the keenest and deepest thinkers on nature from an immeasurably increased and perfected knowledge. The interval between what is known now and what Harvey knew is much greater—greater as regards extent of knowledge, still greater as regards its precision and comprehensiveness—than the interval between what Harvey knew and what Galen knew. And, therefore, it might appear likely that there was ground for an instructive comparison between methods and interpretations current and natural at an earlier stage of physical inquiry, when it was but beginning and making its first attempts, and those believed to be tenable in the more advanced one.

Harvey is known as a bold and original discoverer who accepted the supposition of final causes in nature. He thought that he found, prevailing throughout it, adaptation, and what in human language is called contrivance; he inferred purpose and design; and he expected to meet with ends explaining the existence of facts, wherever he investigated. He was not, of course, singular in that. He held this view in common with all religious thinkers, most of whom knew next to nothing in reality of nature, and in common with many theorisers on natural philosophy, who were led by it to content themselves with the most slovenly and unverified guesses instead of knowledge. But Harvey was an open-eyed, resolute investigator of things as they are, hard to satisfy, and stiff in refusing to take up with loose explanations. He was not merely a man of ingenious suggestions and brilliant presentiments: of him it may be said emphatically, with reference to the discovery which has made his name famous, and which more than one clever predecessor had felt after and almost seized, but had failed to verify, that he only discovers who proves. His observation was, Dr. Acland says, extensive, shrewd, cautious, and patient; and his notion of proof was full, comprehensive, and strict. Dr. Acland quotes some extracts, in which Harvey describes the mode of reasoning on which his doctrine of the circulation was based; and nothing can be a better illustration in practice of that combination of vigilant and inquisitive watching of phenomena with inventiveness of imagination and idea, checked at every step and consequence by the rigorous and inexorable test of ascertained fact, which Lord Bacon so eloquently illustrated in his writings and so imperfectly exemplified in his physical studies. Harvey's language is not unworthy of Lord Bacon himself, when he meets the objections against his discovery. To those who cannot see the efficient or the final cause of the circulation, and who exclaim *Cui bono?* he answers—

> "I own I am of opinion that our first duty is to inquire whether the thing be or not, before asking wherefore it is; for from the facts and circumstances which meet us in the circulation, admitted, established, the ends and objects of its institution are especially to be sought."

To those who object to his explanation that it still leaves much unexplained, or that it is against all that has been hitherto thought, he replies, with emphatic solemnity and loftiness of conviction—

"To all these I reply; that the facts cognisable by the senses wait on no opinions, and that the works of nature bow to no antiquity; for, indeed, there is nothing either more ancient or of higher authority than nature."

Harvey, if any man was, was an observer and a reasoner from observation. Yet Harvey undoubtedly took largely into his methods of inquiry the consideration of final causes. Dr. Whewell says, that they directed his researches and led to his discoveries; and there can be no doubt that at various stages of his investigations they presented themselves to suggest both the question and the solution on which the successful termination of the inquiry depended. Was Harvey, asks Dr. Acland, in giving entrance into his thoughts, among other aspects of a subject, to the consideration of its final cause—that is, to its supposed design and purpose—following a baseless and misleading perversion of the true idea which nature ought to impress on the mind of a scientific student?

Dr. Acland quotes two sharply contrasted answers to this question in its general form. "The idea of a Final Cause is essential as a guide in the progress of biology," says Whewell. "The hypothesis of Final Cause and of all Providential Government is ruined irresistibly by the evidence of modern science," says an equally great authority, Comte. As Dr. Acland observes with truth, Comte unwarrantably joins together, and puts on the same footing, two questions which are perfectly distinct and stand on entirely separate grounds; for a theory excluding final causes,—either on Descartes' ground, that it is impossible to know and rash to guess at the purposes of God, or from preference either for the idea of a creation on the law of types, or for the hypothesis of development and modification by natural selection, or in any other way,—by no means carries with it the denial of appointed order, harmony and unity, or takes refuge in the conclusion of chance as its inevitable consequence. The doctrine of final causes, as Dr. Acland observes, "so far as it involves the idea that we are *able to state the object for which things exist,* may be ruined;" yet it does not follow that we must necessarily infer, with Comte, the absence of providential government: and as a matter of fact, many writers who are jealous of the use of final causes in science do not disavow design, though they doubt their own powers of reading it, and "admit, indeed require, the presence of a superior operating will, of which we recognise the results while we cannot divine the motives." But though Comte mixes up two questions with a very unphilosophical *animus,* he yet undoubtedly states with point and force what is the judgment of a large body of modern scientific opinion on that ancient philosophical conception of nature which assumed in it distinctly intended aims, uses, and instruments, capable of being discerned and reasoned upon by the human mind; a conception on which Harvey undoubtedly acted to a great extent.

Comte, then, pronounces absolutely against the notion of final causes, as an irrational notion which breaks down under the examination of phenomena carried on with the precision and largeness of comparison of modern science. Will his own sweeping generalisation bear the test of accurate reference to fact? With characteristic boldness, he does not shrink from the trial in what seems an extreme case; and he points a warning against blind admiration of the skill and perfection of natural arrangements, by alleging, among other imperfections of the structure of the eye, the "fundamental uselessness" of the crystalline lens—"as if there were much wisdom in introducing inopportunely a part which is not indispensable to the phenomenon, and which may nevertheless in certain cases prevent it entirely." This may be taken, as Dr. Acland says, as a crucial instance to prove either that there is no design, or that the design, if it exists, is bad. Lucretius had maintained the doctrine, that eyes were not made to see with; but that, as we happen to have eyes, and eyes happen to be suited to the transmission of light, we see. But Lucretius spoke loosely, on general grounds, just as much as the Stoic or the Platonist, and without knowing anything in detail about the structure of the eye. Comte, with modern anatomy and optics before him, pronounces the crystalline lens to be, first, superfluous, and next, a gratuitously additional source of possible disablement. Here is a question of fact, which, in any argument raised upon it as to the further inference of design, ought first to be stated correctly. Dr. Acland joins issue on the fact, and shows beyond a doubt how loosely and carelessly Comte has treated the facts from which he so peremptorily generalised. Dr. Acland's examination of Comte's allegations is clear, exact, and directly to the point. First, as to the assertion that the lens is unnecessary, he observes that, roughly speaking, the allegation is true. "Though we could not see quite well without it, still we could see. The crystalline lens is not an essential part of an eye, abstractedly considered: it does not, therefore, exist in every case where there is an eye." But only roughly speaking. Comte's remark is so far justifiable, that the lens is certainly not necessary for the transmission of light, nor for bringing it to a focus, if the focus be a fixed point—that is, for objects at a fixed distance. Eyes are constructed without it for objects at one distance; and eyes for one distance may be supplemented, as in the case of insects, by additional eyes for another. "But the use of the lens in man, as the highest type, has been settled in an unanswerable manner." Man needs not only power of sight at fixed distances, but a power of adjustment for varying ones; and the only mode of adjustment known is by the lens. The lens, by alteration of focus, adjusts the eye to distinct vision at varying distances; and it does so, obeying the intention of the mind without effort on the part of those who use it. This use, suspected by Dr. Young, has been proved by later inquirers, and is even capable of demonstration by their delicate instruments. To rejoin, as Comte was inclined to do, that it could have been better done by man's skill, is the most idle childishness: till it has been done, the only serious question is, whether means and end fit each other exactly? Next, as to its liability to derangement, Dr. Acland observes with truth, that "the attack is, deprived of its sting if the organ be necessary. The sting is, that a structure, which at best is useless, is placed in the middle of a delicate instrument; that when itself is injured, it destroys the operation of the necessary portion; and that it is often injured." The question, of course, is not whether the lens is, like everything else in the world, liable to mischief and decay, but whether there is more disease and failure in it than elsewhere; whether the disorder which it directly brings with it is so great and special as to overweigh the benefit of its presence. And he points out that the true account of the case is this: first, that the lens is, to speak in human language, one of the most hazardous and daring expedients conceivable; exceptional in its structure and complicated function, as well as in its purpose; requiring mathematical precision in point of figure, specific gravity, and refractive power;—requiring, along with this, power of growth and nutrition after formation, and alteration of figure and accommodation; and irreparably impaired or destroyed by the most delicate loss in figure or power of nutrition; by alteration in transparency, index of refraction, or elasticity in ever so small a degree: and next, that this exceptionally and perilously delicate instrument, "which suggests the feeling of helpless wonder that it can be either made at all, or, when made, that it can be maintained," is in fact guarded by protective arrangements to a greater extent than any other structure of the body; and that the qualities on which its use depends "are and have been for untold ages secured by the contrivances described in millions on millions of individuals at any moment of time."

Dr. Acland's comments on this negligent rashness of generalisation are surely no more than just. Comte criticises the structure of the eye to enforce a general theory of nature; and, speaking as positively as a theorist can, he has not taken the trouble to make sure of the facts which he alleges; he misapprehends and misstates them. "An objection such as Comte's is, it is not too much to say, an instance of a dogmatic assertion as arrogant as any ever made by religionists." It is certainly not more excusable in a professed votary and follower of nature than in one who is biassed by another great side of feeling and thought. "The order of nature may be," as Comte says, "very imperfect in every respect;" but it is simply impossible for us, with our human capacities, and laws of thought and intelligence, to examine and discuss it exactly, without being obliged to speak of what we find, in the terms which we borrow from our way of speaking all that is most excellent and admirable in our own adaptation of means to ends—of the harmony between instrument and operation, of the triumphs of great ideas realised and great purposes fulfilled. We cannot talk of the eye as it is, even if we think that we can improve upon it, without feeling and showing ourselves conscious of something perfectly different from an admiration of the *chance* wonders and beauties of nature—that is, without being gratified in an intellectual sense of *skill.* It is impossible to get rid of the persistent and obstinately obtrusive presence of suitableness, adaptation, success dependent on the means prepared to attain it.

Are we to shut our eyes to this? For this is really the meaning of the question, whether we are to see, and to look for, when they seem to offer themselves and invite our notice, final causes in nature? It seems to be really just as unphilosophical to resist the evidence which crowds on us suggesting and pointing out purpose and design, as it undoubtedly is to see them where they are not to be found, and to bring them in by force or invention where nature does not show them. In answering the question about Harvey and the men of his day, like Boyle —who gave a place, but not an exclusive or governing place, in their philosophy to final causes—whether in so doing they violated sound scientific method, Dr. Acland wisely and justly maintains that they were guilty of no fault in philosophy. Final causes are mischievous when they dispense with inquiry, or when they eclipse or distort facts. But Harvey's belief in God, causing him to anticipate order and purpose in nature, did neither. His supposition of final causes, which led him to infer the purpose of the valves of the veins, giving passage to the blood one way and stopping it in another, was really but the exercise of "that common sense without which no investigation is possible." And the question of final causes when the facts are clear and ascertained is, so far as the case goes and we can speculate upon it, a question of common sense. But when Harvey went on to speculate on the final cause of the chick, no doubt he passed into the regions of theory and abstract conceptions; yet, unless his theory cramped and rendered unfaithful his actual observation of phenomena—which there is no reason to think that it did—though we may not agree with his way of putting his case, we have no right to call his proceeding unphilosophical. It was simply that he had come to the transcendent stage of scientific inquiry, just as Comte, or Humboldt, or Darwin, or Owen, come at last. Reason, guided and informed by experiment of course, but reason, must equally be the guide of all speculations such as these; and he constructed his theory of nature on one set of suppositions, which of course are open to question, but not more open to question than all such suppositions must be. The idea of types in nature is as purely a product of the mind, an application to facts,—which are what they are, whether it is true or not—of its own familiar ways of seeing things, an expedient of the reason to enable it to see order, connection, and meaning in apparent confusion,—as the idea of design. Harvey started from the idea of God—that idea which the spectacle of nature confirms, though it does not originate; and which it confirms, according to a striking passage of Kant cited by Dr. Acland, not so much in a strictly scientific form by the detailed evidence of scientific fact, as by the broad, open display of utility, magnificence, and glory which meets the eyes of all men who look abroad on the world which they dwell in. Harvey started with the belief in God; and that belief led him to accept the supposition of order, harmony, design, and arrangement ordered from design, throughout nature. He thought that he recognised, as most men will ever think, that the eye was made for seeing, and the ear for hearing; and in the course of the reflections which issued in his great discovery of the circulation, he looked to see whether the structure of the heart and its accompanying machinery was calculated for that circulation. He went a step further, and supposed the ideal existence of the created work in the mind of the Creator. If there is a deeper philosophy to be had, it will in due time supplant his. Meanwhile, the place which he gave to final causes in his conception of nature never tempted him to unphilosophical and idle speculating—never made him indifferent to the truth of what is, or an impatient and untrustworthy reporter of it. We have no right to call any man unphilosophical who faithfully and seriously checks his philosophy by fact and experiment. His philosophy may, no doubt, in the course of time need to be widened or corrected; but if it honestly rests on facts so far as it goes, it is on the way to be so.

It is thus that we understand Dr. Acland's generous and appreciative judgment on one of the noblest of the old pioneers of English science. Harvey was not irrational and erring in his thoughts of nature, because he had not yet attained to the wider view that is opening on our eyes. He believed in final causes, and our progress in knowledge is very far indeed from entitling us to lay down as a principle that final causes are unscientific. A view of nature confining itself to the conception of final causes is, no doubt, inadequate to explain the phenomena of the world; but so, on the other hand, is such a view as Comte's, which excludes them, and has to force the natural look of things in order to exclude them. For, do what we will, we cannot withdraw from our eyes the importunate crowd of instances which exhibit to us a world proceeding, in great things and small, "by measure, number, and weight;" we cannot speak of its goings on, except by the help of words which imply design, preparation, pre-appointment. With such a spectacle before our eyes every moment, the mind of man finds it impossible to reconcile itself to surrendering, even in obedience to science, the common-sense idea of final causes; and till science knows more completely than it yet does, or seems likely to do, it has no right to make the claim.

The unity of plan and order in nature is the need and conclusion both of reason and religion; but it is not the *prima facie* and inevitable suggestion of phenomena by themselves. Great tracts and divisions of them seem to speak of different laws, and countenance different interpretation. The cypher seems to alter, and the key which fitted it in one place fails in another. We find over a great portion of them the prevalence of what we call contrivance. The presence of final causes, of means and design, seems manifestly to reveal itself far and wide, and in what we are most familiar with. Yet, equally manifestly, we come to a point where we can trace them no longer. Design does not seem to be the exclusive principle governing all nature. Besides it, is disclosed, with equal evidence, the principle of type: we see organs suggestive of definite use and design come to be useless, or applied to another purpose; means apparently intended for one end, strangely and anomalously developed to serve equally completely another. We seem to see—at least, by so viewing things the conception of their history and meaning becomes easier to our intelligence—mere powers, left to wander and stray, and find their employment; which have, as it has happened, met with their use and application, not by any original law, but by the pressure and direction of surrounding circumstances, and have developed as they have done in obedience to what to us seems accident. Lastly, we come to much which we can explain neither by design, nor by the analogies of form and type, nor by ideas of development; things having their end in themselves, amenable to no theory which satisfies our reason, and, for the present, simply refractory and unaccountable. How strange, in our assemblage of powers, is the sense of smell, with its refinements of pleasure and pain! how still more strange the accompaniments, through the whole realm of nature, in plant and animal, of the process of reproduction, so simple in its essential features, so infinitely varied in its accessory forms, with all its extraordinary concomitants of grace and ornament, of what is grotesque and repulsive, with all its extraordinary collateral influences and effects on the course of things! Yet why, after all, should we have a narrower idea of the working of the great Doer than of man, whom we believe to be his image. All in man is not merely ends, means, uses, arrangements. There is the sense of beauty and the inexhaustible tendency to inventiveness and creation for its own sake. There is the delight in variety, and all the unuseful feelings and habits. The reasons are equally strong, but absolutely incommensurate, for which he admires a steam-engine or a microscope, and, on the other hand, a picture of Raphael or a sonata of Beethoven. Man works for use on the whole, and the stamp of use is on his doings; yet he thinks and works not for use only. But most certainly, when we try to construct a philosophy of nature on any exclusive aspect of it, whichever line we follow, we are soon brought up by facts. Design and contrivance will not carry us to the end of nature; but if we cast away contrivance and design, we are soon landed in paradoxes like those of Lucretius. So, it is undeniable that nature lends itself to an irreligious as well as a religious theory of it; but it is to be observed that evil and failure—the purposeless, the capricious, the parasitical—are the weak points of the religious theory, and that success and achievement, good and beauty and happiness, are the weak points of the irreligious; and there is no comparison in degree as to the mass and amount of positive facts which each theory finds in its way, and has to make consistent with its hypothesis. It is not easy to account for the fact of evil; but it is a light task compared with that of accounting, on the irreligious theory, for all that nature teems with of good. But nature herself will not reveal her riddle. She is passive; and it is not nature, but the mind and reason of man, faithful at once to himself and faithful to what he sees, which must supply to man the only answer he can get of what she is. Besides nature, there is man in the world. Nature, *without* man, might suggest a mere blind succession of phenomena; but what we find as a fact, is nature *with* man. It is from himself that he gets the idea and the faith of a First Cause, of government and law, of God; and that idea is independent of nature. He cannot know it, he cannot verify it, as he can facts of nature; but he would be crippled and maimed indeed, if his knowledge, as we speak of knowledge in science, were the measure of his sympathies and his reason, his hopes, his trust, his convictions. He rests on something different from actual knowledge in his belief of God, but it is what is analogous to that which is the ground of all his moral life. But, starting from the belief in God, he has no reason to be perplexed at the difficulty which he finds in bringing all the various aspects of nature, so to speak, under the same formula of a uniform generalisation. It is, probably, impossible at present to bridge over by definite steps the interval between the First Cause and his works. But the lines of connection which we can see are neither few nor indistinct; and to refuse to trace them because they are broken and incomplete is to be false to our own powers of insight and interpretation; of seeing meaning in indications, and a plan though only shown in fragments. In our present incomplete state of knowledge, the great thing is, to see what is before us as it is; not to persuade ourselves that we see what we do not see; but neither to refuse to see what we do see, because it *seems* to involve our seeing something else which we do not see. Harvey talks of final causes in a phraseology which is to us antiquated and unmeaning; but when he traces purpose and design, they appear palpable, and it seems only common sense to recognise them. Comte, with obvious and striking utility and instrumentality before him, makes difficulties about seeing it, and compromises his own character for knowledge, because, not being able to carry the principle of final causes all through, he thinks it necessary to generalise against it altogether. He will imperil science rather than give up his universal. It seems to us that Dr. Acland is right in thinking that the older inquirer had the larger and more open, as he had the more religious, mind.

Review VI.

On Diseases of the Veins, Hæmorrhoidal Tumours, and other Affections of the Rectum. By Henry Lee, F.R.C.S., Surgeon to St. George's Hospital, &c. &c. Second Edition. Churchill & Sons. 1866. Pp. 190.

The work before us consists of two parts: the first, a treatise on "Diseases of the Veins;" the second, an essay on "Affections of the Rectum."

Mr. Lee published his Jacksonian Prize Essay on the former subject in 1850; and he not only advanced our knowledge, but the progress then made was in the right direction. He was one of the first, if not *the* first writer, to divert our attention from the morbid changes in the *coats of an affected vein,* and definitely fix it upon *the altered condition of its contents;* and all subsequent researches have been in the same direction. Since that time the whole subject of phlebitis and its allies has been re-opened: many old views have been discarded; old facts now possess a very different significance, and have received another interpretation, and a new pathology has found its expression in a new nomenclature. There have been many scientific labourers in the field, but it is mainly to Virchow that we owe the revolution which has taken place. To an ordinary, to a casual professional reader, there is no subject more obscure or more difficult than this one of diseases of the veins. If he turns to an article on "Pyæmia" or "Phlebitis," he will probably be as much bewildered by the different and conflicting views, as by the new terms which have been so plentifully introduced of late years. We do not wonder, then, that the second edition of this essay required to be re-written.

A great many morbid processes having no necessary relation to disease of the veins still retain a loose connection with them. Sometimes the connection is maintained with scientific accuracy, but, generally, this is entirely sacrificed for the purpose of accommodating a number of collateral matters.

In the second edition of this essay we recognise the "bones" of the author's old doctrines, clothed in the new garb which our recent advances have rendered necessary. It would be tedious to follow the author in his review of the doctrines which have prevailed at different times, but some reference to a past pathology is necessary in order to appreciate its altered aspect in the present. It is, we fear, hopeless to expect that any one will clear the subject of all extraneous matters, and point out in a lucid way what does, and what does not, belong to diseases of the veins. John Hunter seems to have believed that the injurious consequences which occasionally followed inflammation of the larger veins were to be attributed to the propagation of inflammation along the lining membrane of the vessels, and especially by its extension to the heart. His attention appears to have been directed in an especial manner to abscesses of the lungs, and to the condition of the veins in their immediate neighbourhood; but he does not seem to have entertained the idea that these appearances could have been connected with an alteration of the blood, by the entrance into the vessels of some morbid product—from a wound, for example. He pointed out that the blood was almost always coagulated in inflamed veins;

but sometimes such coagula were not present. He then believed that, if purulent matter formed in a vein, it might become mixed with the blood and carried in the course of the circulation. Mr. Arnott was the first person who proved that the signs of inflammation and irritation are by no means generally found in the heart or larger vessels. He conceived that pyæmia resulted from inflammation of the lining membrane of a vein, its secretion of pus, and the introduction of that secretion into the circulation. M. Cruveilhier, in his 'Traité d'Anatomie Pathologique Générale,' constructed a theory to account for pyæmia and metastatic abscesses. According to this, such affections took their origin from a suppurative phlebitis, and the introduction of pus into the current of the circulation; and the purulent matter, on being conveyed to the capillaries of an internal organ, excited a capillary phlebitis, with circumscribed inflammations and suppurations of the neighbouring parts.

The searching criticism which this pathology has undergone, the vigour with which Virchow attacked Cruveilhier's position and exposed its weak points, are now matters of history. Suffice it that Virchow has left Cruveilhier's construction a venerable ruin. When Mr. Lee so emphatically asserted, in 1850, that the swelling and thickening of a vein, in so-called phlebitis, were caused by the contents and not by the coats of the vessel, he just touched with the tip of his finger a fact which another and a master-hand has since grasped.

Perceiving the very frequent occurrence of a localised blood-clotting, unconnected with the phenomena of inflammation in a vein, Virchow seized upon that fact, and followed out all the local and remote effects to which it might give rise, and thus made it the basis of one of the most brilliant pieces of pathology of the present age.

Virchow's views have been much misapprehended and misrepresented in these days of rapid and careless readers. His lectures, as translated by Dr. Chance, are models of style and perspicuity in their way; but they are so full of thought and observation, that a rapid reader is peculiarly apt to carry away an inadequate idea of what he has and has not said. He is sometimes almost represented as if he had denied, what he takes great care to assert—viz., the existence of such a disease as phlebitis.

As our author asserts, coagulation of the blood may sometimes be the cause and sometimes the consequence of inflammation of a vein. The main interest, however, centres in the clot—in the changes which may ensue in it—in its liability to break up, so that some portions of its substance may become transferred to other and distant organs through the medium of the circulation.

All authors agree as to the existence of phlebitis, the difference of opinion being as to the occurrence of an inflammation of the lining membrane only. Mr. Lee repeats the result of his observations, published in the thirty-fifth volume of the 'Medico-Chirurgical Trans.,' in order to show that the lining membrane of veins does not comport itself, under the effect of irritants, in the same way as the other serous membranes do; and that local irritation manifests its effects upon the blood itself, and not upon this membrane. Between affections of the venous system and pyæmial disorders there is unquestionably some intimate connection. It is due to this connection that operations upon, or injuries implicating, veins are so dangerous. It is chiefly through the thin-walled veins, such as those of bone and the uterine sinuses, that such danger notably exists. In most cases, the advent of pyæmia appears to be connected with the exposure of the blood in these vessels to the action of disordered secretions from contiguous wounds; but in some, we still think that a veritable suppurative phlebitis actually forms the starting-point. Although we may fail to demonstrate the signs of this, that is no proof of its non-existence; for how often are there unmistakable symptoms of inflammation in the mucous membranes of the stomach and bronchi during life, and yet how little may be presented for the morbid anatomist! In the veins it must be still more difficult to trace any evidences of inflammatory action, because the circulating blood will remove all its products; and, if coagulation ensues, there is no method by which we can trace pus-cells in the coagulum, or differentiate them from the white corpuscles included in its meshes. As a case in point, we distinctly remember finding a fluid in the ophthalmic veins possessing all the physical, chemical, and microscopical appearances of perfect pus. The patient in this instance died of pyæmic infection after erysipelas and abscess of the face.

The tendency of the present day being to ignore altogether the existence of phlebitis affecting the lining membrane, we extract the following from Mr. Lee's work:—

> "The coagulation that is the effect of a phlebitis seems to be occasioned by some morbid impression made upon the blood through the lining membrane. The clots which are found firmly adherent to one side only of an artery or vein are, almost invariably, connected with a diseased condition of the vessel at that part, and are the result of some localised irritation. Although it is undoubted that the internal coat is not liable to be inflamed in the same way as serous membranes are, there is no reason why it should not be affected with a perverted nutrition allied to a state of inflammation.

The coat being a non-vascular one, and composed of very delicate epithelial cells supported on a fine substratum of connective tissue, we cannot expect to find the phenomena identical with those in inflamed vascular tissues; changes in the structure and contents of the cells, marked by a cloudiness or opacity of the epithelial layer, and the production of very fine and elementary growths in the connective tissue, here take the part of that lymph and false membrane which are the results of an inflammation of the peritoneum or pleura. The researches of Mr. Lister and others sufficiently indicate what very slight deviations from the normal relations existing between the membrane and the contained blood may lead to a coagulation of the blood within a vein."

Our author proceeds to consider the vitiated fluids which may enter the veins; the nature and composition of pus, and the products generated by its decomposition; the relation of wounds and local lesions to pyæmial diseases; and he refers to the experiments of Panum, Weber, and Polli. He then goes on to treat of his subject under three heads.

The number of vitiated fluids which may traverse the veins without leaving any trace of their course is probably great; but there are some which leave evident indications of their passage towards the general circulation. With the latter class we are principally concerned in relation to phlebitis. These morbid agents may practically be divided into three classes:—

1. Those which, on admixture with the blood, determine its coagulation.
2. Those which do not produce a direct coagulation of the blood, but have a tendency to cause the fibrin (in combination with albumen) to separate from its other elements, and to deposit itself in some part of the circulating system.
3. Those which, upon admixture with the blood, produce its decomposition.

With reference to senile gangrene, he is disposed to regard it as a chronic process, and as caused by the disintegration and entrance into the circulation of fatty, calcareous, and other morbid deposits from the coats of the vessels, with any little portions of fibrine which may have temporarily adhered to those parts where the lining membrane has given way.

That part of the subject which relates to the decomposition of blood in living vessels is one of the most important and interesting. If once commenced, it may rapidly be communicated, either by mere contact, or by the removal of the decomposing blood or clots, to a distant part of the circulation. From some experiments Mr. Lee draws the conclusion that, when very putrid substances are introduced into the vessels, the tendency to coagulation in the blood may be quite insufficient to prevent their circulation through the general system. The blood then becomes altered in composition, and may even be deprived of its coagulating property altogether. Not only the fluids, but the so-called solids, pass rapidly into decomposition; and the former would appear to have the property of infecting other parts, and even other healthy bodies. These effects are, he thinks, very different from those of thrombosis, embolism, and metastatic abscesses. In connection with his subject he adverts to those osteo-myelitic affections which the French military surgeons, Dr. Valette, M. Jules Roux, and others, have described. The description of the morbid changes induced in various organs by the existence of a general infection of the blood is shortly but well given.

The complex causation of pyæmia is not generally sufficiently borne in mind. We have to consider—

I. The mechanical effects arising from the presence of disintegrating blood-clot in the system; the morbid processes induced in the solid viscera and other parts, where fragments of such coagula are conveyed to, and lodged in, the minute vessels distributed to them.

II. The symptoms of blood-poisoning set up by the action of morbid or ichorous fluids.

According to the preponderance of one or other of these, according to the proportion in which these are combined, so will the effects vary. In the first, the phenomena following thrombosis and embolism ensue—the localised coagulations, effusions, inflammation, abscesses, or gangrenous softenings. The nature and activity of these different lesions must depend, to a great extent, upon the source and nature of the original coagulum. In the second, the ichorous fluids act as a blood-poison, originate a series of subtle changes, and produce death before, if not without, the manifestations of those local lesions observed in the first form. The symptoms during life are such as indicate the impression made on the nervous and vascular centres by the circulation of a *materies morbi;* the appearances after death being manifestly insufficient to account for the nature of those symptoms, or the fatal issue.

III.—These ichorous fluids may induce such changes in the blood as to completely unfit it for the nutrition of the tissues. The vitiated blood no sooner reaches the capillaries or small vessels of an organ than a stasis and inflammatory reaction ensues, in the place of the normal inter-changes which should take place between the blood-current and the tissues. Supposing the agent by which the blood has been affected to be a putrescent substance capable of causing coagulation, this property may again come into play in distant vessels; nay, for all we know, it may act as a ferment or catalytic agent, and generate its like out of the materials with which it is in contact.

In the treatment of phlebitis our author advocates an operative measure, which appears to have been suggested to his mind by some observations of Hunter's. As the dangerous symptoms—the secondary metastatic deposits—of phlebitis proceed through the contents of the vein, generally in a more or less coagulated state, and not through a continuity of the diseased action along its coats, he thinks an attempt should be made to prevent the evils arising from the disintegration and transference of portions of healthy or decomposing coagula to other parts. By dividing the vein sub-cutaneously above the seat of disease, so as to secure a permanent union of its coats, he conceives, we must impose an effectual barrier to the entrance of clots or decomposing fluids into the circulation. If, therefore, there was evidence of phlebitic disease in a superficial vein, such as a local coagulation of the blood produced by injury of the vein, or by the action of vitiated fluids absorbed from a wound, Mr. Lee would subcutaneously divide the trunk of the affected vessel at a higher point, and apply acupressure needles.

This suggestion may be a very good one where it is practicable; but it too often happens that general contamination of the blood ensues through very minute vessels hidden from our sight, and no suspicion of pyæmial disease arises until constitutional symptoms have manifested themselves.

The author refers to the use of sulphites in these forms of disease, and evidently thinks them well worthy of trial; but he takes especial care to avoid all eulogistic or exaggerated terms in advocating Dr. Polli's remedies.

A chapter on varicose veins, and their radical cure by subcutaneous section, and the application of acupressure needles and elastic bands, follows. Next, one describing the results of a similar procedure for the cure of varicocele; and this completes the section on diseases of the veins. We can understand the efficacy of this method for the radical cure of varicocele, because the operation includes the great bulk of the veins involved; but the explanation is not so easy in the former case. In our experience, varicose enlargement of the superficial veins is only a part of the affection after all: while the altered state of the superficial veins of a limb is appreciable to our senses, there remains the dilatation of another set of veins, commonly present at the same time, and these are too deeply placed to be seen or reached.

The second part is devoted to diseases of the rectum. The section on the surgical treatment of hæmorrhoids will be scanned with interest at the present time. Into the relative merits of the ligature, nitric acid, or excision combined with the actual cautery, we need not enter. Our author evidently prefers the two last-named methods of cure; and his opinion is entitled to consideration, because it is based upon his own experience. In order to guard against hæmorrhage after the excision of internal piles, Mr. Lee devised an instrument in 1848 for clamping the part. He carefully indicates the kinds of hæmorrhoidal affections for which the nitric acid alone is applicable, those for which excision in combination with the nitric acid or actual cautery, and those for which the ligature is best adapted.

The chapter on Fistula Ani is a very good one. In the remaining portions of the book the author treats of such other affections of the rectum as commonly come within the observation of an hospital surgeon.

A few words in conclusion. This book bears the marks of having been hastily put together, and it manifests the disadvantage under which the author laboured when he had to rehabilitate and adapt an old essay to the present state of knowledge. The first part contains many interesting observations connected with the pathology of diseases of the veins, and some very practical suggestions for their treatment; the last supplies us with the author's more matured experience of the more common affections of the rectum, with a clear account of those remedial and operative measures which he has found to be the most reliable and efficacious in his own practice. It is a good book, containing, in particular, much that is very useful and practical on the subject of varicose veins and diseases of the rectum.

REVIEW VII.

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2. *Rapport sur les Hôpitaux Civils de la Ville de Londres, à point de vue de la Comparaison de ces Etablissements avec les Hôpitaux de la Ville de Paris.* Par MM. BLONDEL et SER. —*Paris,* 1862. pp. 238.

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10. *Polizia degli Spedali.* Scritta dal Dottor GIOVANNI POZZI. —*Livorno,* 1839. pp. 378.

On Hospitals. By Dr. JOHN POZZI.

IN the January (1866) number of this Review, the questions of site and of the general arrangement of hospitals were discussed, and a preference given to country or suburban sites, and to the pavilion system of construction. Upon those questions most discussion has arisen; but there are many others, relative to structural details, concerning which there is also great diversity of opinion. Happily, there are some principles of construction respecting which a general agreement may be predicated. Among such are the complete separation of the wards or apartments for the sick from the quarters occupied by the officers and servants of the establishment other than nurses; and from the kitchen, laundry, and other general offices connected with the administration; the disposition of the pavilions or wards, so that, as M. Giraldès remarks, they may at all seasons be bathed in sunshine, and swept by the wind and rain; the elevation of the buildings upon well-drained foundations, and the placing of all drains at some distance outside the walls; the construction and fittings of the interior of the wards to be such as to secure thorough ventilation and adequate warmth, and to obviate nuisance from accessory apartments, water-closets, sculleries, baths, &c.

However, when we descend to particulars, and the problem is submitted how best to carry out these generally accepted principles of construction, we again encounter considerable differences of opinion. Some of the principal questions touching the construction and internal arrangements of hospitals for the sick we will now pass under review.

The first demanding notice is that of the dimensions to be allowed to an hospital. In former times, the only recognised considerations to limit hospital extension were, the demand for accommodation and the possibilities of the space whereon the building was situated. In more recent times, the persuasion has taken hold of many, that the enlargement of establishments for the sick cannot pass beyond a certain point without detriment to their sanitary state, nor without disadvantages to their administration. Beyond that point, they become assimilated in kind to small towns or villages ravaged by sickness; the aggregation of sick folk becomes too great, and a morbid atmosphere is engendered around the institution. The overgrowth of hospitals is especially seen in several cities of the Continent; but it is impossible in their case to estimate what detriment may arise from it alone, inasmuch as more potent injurious conditions obtain in them connected with their construction, with overcrowding of wards, &c. In this country, hospitals for the sick have not attained nearly such large dimensions; but there are two or three asylums for the insane well-nigh rivalling some of the largest Continental hospitals; and, so far as experience with them is at all adducible on the point at issue, it would sustain the position that the aggregation of sick in large numbers is fraught with disadvantages to their physical and mental condition. At all events, the reports of the huge asylums of this country appear to prove this much:—that their great size augments the difficulties of management and the cost of maintenance. Miss Nightingale (op. cit., p. 64) thus expresses her views:

"An hospital may be constructed for any number of sick, until a point is arrived at when some portion of the administrative arrangements, material or personal, has to be provided in duplicate. Any further extension beyond this ceases to be economical. Considering each pavilion as a separate building in the hospital construction, any number of single or double pavilions could be put together up to accommodation for, say 1000 beds; beyond which it would be difficult, if not impracticable, to have good administration with one set of officers. It is to be hoped, however, that few hospitals will ever be built for such a number now-a-days. The fewer hospitals required, and the smaller their number of sick, the better will it be for civilisation. All I submit is, that the pavilion construction may, not should, be safely used up to this extent."

Dr. Bristowe and Mr. Holmes ('Report,' p. 509) proceed further than the writer just quoted, asserting that the size of an hospital is a matter of absolute indifference, provided that

> "all the subordinate parts have been well constructed for ventilation and space, and all are kept in perfect order by drainage and cleanliness;"

that with such provisoes the limitation of the number of patients under one roof to 100 is uncalled for; and that,

> "allowing that each ward is healthy, and that, say thirty patients, may be safely placed in the same ward, they see no reason to doubt that it is just as safe to put 100 wards together, and accommodate 3000 patients in the same hospital."

M. Blondel ('Rapport,' p. 50) fixes the number of beds at 400 to 500, as that best adapted to secure efficiency in administration with the greatest economy; and in this particular coincides with M. Trélat ('Discussion,' p. 8), M. Giraldès (ibid., p. 36), and M. Léon le Fort.

The conclusion arrived at by the Surgical Society of Paris was, that

> "Satisfactory hygienic conditions are readily attainable in hospitals accommodating from 200 to 250 sick. These conditions it is well-nigh impossible to realise in great towns where the hospital inmates exceed double this number. Within the range mentioned, the expenditure of every sort does not surpass that of the more populous establishments." (P. 135.)

With several of the members, great preference was shown for small hospitals; and M. Legouest attempted to demonstrate by the statistics of the military hospitals in and around Paris, that

> "The mortality of hospitals stands in direct proportion with the number of patients they contain; that is to say, that an hospital

receiving 300 sick would lose fewer in proportion than one containing 600. If the former lost two per cent., the other would lose four per cent."

On the other hand, the two English official reporters assert it as a fact, "that under similar circumstances small and large hospitals show no appreciable difference in mortality" ('Report,' p. 509, foot-note); and they, equally with M. Legouest, appeal to statistics in support of their assertion. These conflicting statements appear to be alike the fruit of insufficient and defective statistical evidence, and we apprehend the truth lies between them and that, *cæteris paribus,* hospitals of the moderate dimensions approved by most of the French physicians referred to are most favorable to the recovery of their inmates.

Of more importance than the last question is that of the space upon which hospital buildings are distributed. Ample light and air must reach every part of such buildings; otherwise the evils of agglomeration of the sick, whether the total number in the entire establishment be large or small, make themselves felt. This subject has been already discussed in the preceding article, and the general conclusion arrived at was, that the pavilion plan of construction presented the best arrangements to secure light and air on all sides. It only remains to observe, that where pavilions are arranged side by side, the space between them must not be less than double their height. The Paris Surgical Society proposed a much wider space, of from 80 to 100 mètres; but buildings when placed so far apart, though hygienically advantageous, involve, as Miss Nightingale justly remarks, "a greater cost for land and a greater distance to be traversed by the hospital staff."

That the Paris medical men insisted on so wide a distribution of pavilions, is in part explicable by the profound impression prevailing among them that much of the insalubrity of the splendid Lariboisière Hospital is attributable to the too close approximation of its several blocks.

Opinions are much divided relatively to the number of floors an hospital building should possess. The writer in 'The Builder' and Miss Nightingale—to whom also may be added Pozzi—insist that not more than two stories should be allowed. M. Verneuil and others, in the course of the discussion at the Paris Surgical Society, expressed the same opinion, and it was ultimately adopted by the Society at large as a rule in hospital construction ('Disc.,' p. 135).

The grounds for this conclusion put forward are—that

> "A community of ventilation exists between the ward below and the ward above by the common staircase, and by filtration of air

upwards through the floor. . . That there is a strong conviction in the minds of careful hospital physicians, surgeons, and nurses, that patients do not recover so well on upper floors. . . Moreover a ***sick population*** requires ***more*** surface for health than a healthy population. And it is clear that if patients are placed on three floors instead of on two, the surface overcrowding is increased by one third, unless the distance between the pavilions is increased in a corresponding ratio. But the general administration of lofty hospitals is also far more difficult and fatiguing than of those of moderate height. Any increase of distance between the pavilions will add to the difficulty; and both difficulty and fatigue are very important considerations for efficiency and economy in this branch."—*Notes on Hospitals*, p. 58.

However, Miss Nightingale admits that the presumed extension upwards from wards below of foul air may be obviated more or less completely by making the floors impervious, and by proper attention to the doors and windows. This being admitted, the expediency of having more floors than two becomes a question rather of administration than of construction; and if the usual rule pursued in public and private buildings of erecting several stories be at all indicative of the prevailing sentiments respecting internal convenience and facility of management, it tells against the assumption that more than two superadded floors are adverse to the efficiency of hospital administration. The fatigue and waste of time involved in "running up and down lofty hospitals," as urged by this writer, must be measured against the like unprofitable labour consequent on traversing the wider space required for additional buildings to otherwise accommodate the same number of patients. And further, much of the labour entailed by loftier buildings may be saved by the construction of lifts. At the same time, it will be generally recognised that more than three floors is, to say the least, undesirable. What Sir Ranald Martin's opinion on this matter may be is not clear, inasmuch as in one place his instructions are, that not more than two stories are permissible, and in another, that there may be three. The suggestion of M. Larrey ('Disc.,' p. 105) has much to recommend it—that the uppermost floor, where three exist, is well suited for reserve wards, or for convalescent, or for separation wards, when contagious diseases require isolation.

The notions that, in well-constructed and well-ventilated buildings, impure and noxious air passes from floor to floor, and that the topmost floors are less favorable for the recovery of the sick than those below, are entirely rejected by Mr. Simon, who remarks[1]—

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[1] 'Report,' p. 67.

"As regards wards kept as wards ought to be kept, I cannot conceive that the several clean items will make a dirty total; and, so far as my present knowledge extends, I have every reason to believe that, *subject to the qualifications I have stated*, a given number of patients may dwell under one roof as safely as under several roofs. Nor, subject to the same qualifications, have I any reason to suppose that patients will not recover so well on upper as on lower stories. Of the various surgical wards in which my own hospital experience has been, the one which I remember with most satisfaction was a top ward which had three stories of wards under it."

These opinions are upheld by the two reporters on English hospitals. They have

"Sought in vain to verify the assertion that upper floors are less healthy than the lower, either by personal observation of the many hospitals in which more than two stories are used as wards, or by the opinions of the hospital authorities, who appear, indeed, to be almost unanimous in the contrary opinion. . . But we believe that one floor is just as healthy as another, provided the wards are of the same or similar construction. That in well-constructed hospitals any one ward, still less any one story, shares in the atmosphere of another, is neither proved nor probable. . . . Yet it may be, and most probably is true, that if the whole atmosphere of the hospital be vitiated by defective ventilation and close aggregation of sick, the upper floors may be more unhealthy than the lower. . . . We believe that even if a current of air could be proved to set from one ward or floor into the wards on a higher floor, the distance and the interchange between the hospital atmosphere and the fresh air which must have occurred in the transit would be sufficient to render the communication harmless; but that no such thing has been proved to exist. We regard, therefore, the construction of wards on third or fourth stories as a question purely of convenience. No floors above the second can be conveniently used without a lift for conveying patients, nor is it quite convenient to use the second floor without such machinery; but these lifts are now in such general use, that probably there will soon be few large hospitals without one."

Though the conclusion be sanctioned, the reasoning, as exemplified in the foregoing extracts employed, is not to us quite satisfactory. But, letting this pass without further comment, those who maintain that the air of the wards below diffuses itself more or less in those above, can appeal to the everyday experience of odours ascending by staircases, and externally through open windows, to the upper floors of buildings, and argue therefrom that mephitic air not cognisable by the sense of smell will do the same.

The whole question respecting the relative healthiness of upper and lower wards really turns upon the constructional features of the hospital—whether these are such as secure ample space and free ventilation in its several sections; whether, as Mr. Simon would phrase it, there be a clean total.

In many of the older hospitals the objectionable practice obtained of placing two or more wards in juxtaposition, so that they directly communicated, and, in fact, together constituted a compound ward.

> "If the pavilions are single (writes Miss Nightingale, op. cit., p. 59), *i. e.*—if each staircase gives access to the end of the pavilion —there should be only one ward on each floor, The pavilion should never be divided so that a second ward or wards is placed beyond the first, to be reached by passing through the first. The reason is, that the floor, however divided by cross wards, can never be other than a single ward; the cross walls only obstruct the ventilation. . . . If the pavilions are double—*i. e.*, if each staircase gives access to a ward right and left of the staircase—provided the staircase be of spacious size and thoroughly ventilated sides and top, two wards may be placed on each floor with safety, and with this great advantage, that administration, nursing, and discipline are all facilitated, while expense in construction is saved; (as) one staircase answers for double the number of sick."

In several of the London hospitals, new wards have been constructed, divided into two by a wall running down the middle, perforated at intervals by doors and other openings, whereby the two sections partake of a common atmosphere. Such compound wards are condemned by most writers on hospital construction, including, besides Miss Nightingale, above quoted, Husson, Blondel, Martin, Parkes; but they are approved by Messrs. Bristowe and Holmes. Husson declares such compound wards to be opposed to hygienic rules; and Blondel adds, that the longitudinal division of a ward inevitably interferes with the proper circulation of air, and cuts off the entrance of sunlight from one half (op. cit., p. 65). Dr. Steele, also, in his recent paper on the new wards of Guy's Hospital,[1] refers to this arrangement as highly objectionable. Other objections are, that the partition causes the air to beat down "the draught upon the heads of the inner row of patients; it also prevents the head-nurse from having that view of her whole ward at once, which she ought to have for proper care of it." ('Notes,' p. 39.)

After premising that the shape of a ward is "a matter of minor importance, provided all parts are well ventilated, and provided that there are no corners or recesses to serve as refuges for dirt and impurities," Dr. Bristowe and Mr. Holmes, in contrariety to the other authorities referred to, proceed to say, that—

[1] 'Guy's Hospital Reports,' vol. xi, 1865.

> "The division of a ward into two compartments, by a wall running down its centre, and perforated by openings establishing a communication between the two ('spinal wall'), is evidently an arrangement dictated by the convenience of the architect, and which is less perfect in theory than one which leaves all the wards independent of each other. Yet the comparative triviality of such considerations is shown by the successful practice which some, at any rate, of those hospitals (so built) have attained, as compared with that of others, where, although a more perfect theoretical shape of the ward has been secured, considerations of far more practical moment have been neglected or mistaken. . . . In other respects it is difficult to understand what disadvantage one compartment can sustain by sharing the atmosphere of the other, provided both are equally pure." (Pp. 499, 500.)

A little further on in their report (p. 507) they again recur to this subject, and, with remarkable confidence in the weight of the above observations, thus write:—"As we have before proved (?), these objections (to wards divided down the middle) are not supported by the results of practice in those hospitals" so constructed; and, in conclusion, demand from those who think differently from themselves, facts in support of their opinion. The like requisition for facts may with equal justice be made upon the reporters. The general reference to the successful practice of hospitals with divided wards partakes little of the nature of absolute facts in support of the conclusion arrived at; nor when it is supported by the statistical figures printed in subsequent pages of the report, is its value really enhanced. In truth, the writers in question have themselves indicated a most important condition, materially modifying the value of numerical statistics when adduced in settling such a problem as that now discussed, viz., those circumstances affecting the welfare of hospital patients other than structural details.

Again, if Messrs. Bristowe and Holmes would insist upon the premiss they started with, that all parts of a ward be well ventilated, it is for them to demonstrate that a ward divided along its length by a "spinal wall" "perforated by openings establishing a communication between the two" apartments, can be thoroughly ventilated, and its corners or recesses kept free from dirt and impurities, particularly as the diffusion of light must be interfered with, and, as presumably, four rows of beds must intervene between opposite windows.

The argument of the last clause of their remarks, as quoted, begs the whole question at issue, viz., the purity of the air in the ward as determined by the perfection of its ventilation and lighting. If the air in the two sections of a compound ward were perfectly pure, and always so maintained, it would certainly be a matter of indifference if it equally diffused itself through them. But, knowing that the emanations from a number of persons, especially sick persons, are inimical to health, the question raised is, whether this or that form of ward is best suited to secure a wholesome atmosphere around the sick.

In short, if the argument that no appreciable disadvantage can ensue from one apartment sharing the air of an adjoining one, "provided both are equally pure," be valid, all the supposed improvements in hospital construction of recent years are only bootless and costly alterations; the lessons deduced from experience, fallacies; and the suites of intercommunicating rooms of many old hospitals more desirable, at least on account of convenience and economy in construction, than the detached and independent wards erected on the pavilion system. But let it not be forgotten, that equal purity means also equal impurity of the air in communicating wards. The subdivision of a ward by a partition wall extended through its length, is a structural device that is incompatible with the model ward as sketched by Mr. Simon in the following paragraphs. ('Rep.' p. 66.)

> "Now, hospital construction has *pro tanto* failed in its first obligation, if the form and arrangement of wards and the distribution and planning of windows be not such that each separate ward (independently of all other wards, and of all staircases, passages, corridors, and offices) shall admit of that kind of ventilation, viz., by windows supplemented by air-holes through the walls, and by chimneys, in its completest possible form. An oblong ward, from twenty-five to thirty feet wide, windowed correspondingly on its two long sides with sash-windows reaching to the top, and windowed in such proportion to wall that between each two windows there shall be the required space for one bed-head, and having its communications and separately ventilated offices at its two ends—this, by common consent, is the form of ward which best of all answers the proposed purpose. And in proportion as this form is departed from, in such proportion sooner and sooner will the limit of the ward's usefulness be reached—the point, namely, at which ventilation will prove insufficient—the point at which typhus will spread from bed to bed, and at which traumatic infections will arise among surgical cases."

The question of the dimensions of wards, until of late years, obtained little consideration.

> "In a great majority of the hospitals of ancient construction, and many of modern date (as Dr. Bristowe and Mr. Holmes found), the hospital is cut up into a large number of chambers or small wards, of from six to eight beds each:"

—a mode of construction almost fatal to efficient ventilation, and fraught with disadvantages both on this account, and from the impossibility of proper supervision and nursing, particularly where acute cases and serious injuries are largely received. The error of the system of construction referred to is admitted on all hands, and there is a pretty general concurrence in Miss Nightingale's estimate that the number of beds in a ward affording from 1500 to 1600 cubic feet to each bed, should be from 20 to 32. Pozzi fixed the number at 40. M. Trélat, however, would reduce the standard to a lower number, viz., from 15 to 20; and his opinion was endorsed by the Surgical Society, and embodied in its resolutions. ('Discussion,' p. 135.)

> "A good proportion for a ward of 20 patients (says Miss Nightingale, op. cit., p. 67) would be 80 feet long, 25 (or 26) feet wide, and 16 (or 15) feet high. This would give 1600 (or 1560) cubic feet to each bed. It would give 11 (or 12) feet between foot and foot, which is not too much where there is a clinical school. It would give an average of 16 feet to each two beds in width. Half the sick are supposed to be on each side of the ward."

The maximum width here assigned to a ward, viz., 26 feet, is less than that fixed by Mr. Simon, as above quoted, viz., 30 feet; and Dr. Parkes and Uytterhoeven lay it down as a rule that a ward should not be wider than 24 feet; but it seems to be generally admitted, that if a distance of more than 30 feet intervene between the opposite windows of a ward, the air is not thoroughly changed. Moreover, the necessary cubic capacity for each bed must be principally obtained by the superficial area allowed; for a deficiency in this space cannot be supplemented by increased height of ward. From 15 to 16 feet is the maximum height to be given with any accompanying advantages in the way of effective ventilation.

> "To show the importance of (surface area), it may be sufficient to state that if a large building, say a church, be selected for a war hospital, on account of its spacious, light, cheerful aspect; if it be measured to ascertain its cubic contents, its height being no more than 60 feet, in such a building the very liberal War-Office allowance of 1200 cubic feet per bed would render it necessary to place the beds on the floor so close together that not even a pathway would be left between them. Has not this, in times past, been one cause of the frightful mortality in these hospitals?" ('Notes,' p. 65.)

M. Verneuil adopted the same illustration in his discourse at the meeting of the Surgical Society of Paris ('Discuss.,' p. 61); and the fact that increased elevation will not compensate for lessened superficial area was further exemplified by MM. Le Fort and Larrey, by the results of hospital practice during the Italian war.

"In the wards, each man (writes Dr. Parkes, op. cit., p. 295) ought to have not less than 100, if possible 120, feet of superficial, and from 1500 to 2000 feet of cubic space;" but small wards should have, as near as may be, 2500 cubic feet per bed, owing principally to the greater difficulty attending their ventilation.

The larger dimensions of the wards of Parisian compared with London hospitals is a circumstance commonly remarked, and as commonly animadverted upon. The actual state of the case in the Paris hospitals is thus set forth by Blondel ('Rapport,' p. 74). Of the 243 wards in the several general hospitals—

65 contained from 1 to 2 beds.	26 contained from 31 to 35 beds.
43 „ „ 3 „ 10 „	10 „ „ 36 „ 40 „
62 „ „ 11 „ 20 „	12 „ „ above 40 „
25 „ „ 21 „ 30 „	

And he makes it appear that more than one half of the inmates are accommodated in wards containing between one and thirty beds. However, it is admitted that in the hospitals of London a larger superficial area is allotted, on the average, to each bed than in those of Paris, although the average cubic space is practically alike in the hospitals of the two cities, being something above 1500 cubic feet.

Although wards with from twenty to thirty patients are suitable to the majority of cases, the necessity for more or few small wards is recognised on all hands. Certain cases and classes of cases require separation, and therefore also the creation of special wards, or, as some would urge, of special hospitals. The conviction obtains universally, that the insane and lying-in women are not proper inmates of general hospitals; and also, the majority of writers concur in the propriety of treating contagious diseases in distinct institutions. The desirability, moreover, of separate wards or of separate buildings for the reception of children and of convalescents, is generally recognised. But besides these larger subdivisions of the sick demanding accommodation of a special character, there are classes of patients for whom special provision must be made in immediate contiguity with the ordinary occupants of hospital wards. The custom of setting apart wards for persons suffering from injuries, or the subjects of surgical operations, is of long-standing and general adoption; and its wide-spread prevalence must be held to demonstrate its propriety, at least within certain limits. For it cannot be ignored that the placing together of a number of individuals with suppurating wounds is fraught with danger to all, by engendering a noxious, disease-producing atmosphere in the ward which may overmatch the counteracting force of active ventilation.

Pozzi recommends separate wards for all severe cases of operation, but other writers restrict themselves to recommending small wards of from one to two beds for the reception of such patients. The best relative position of such small wards in general—whether used for the class of surgical patients just mentioned, or for delirious cases, or for such as are plagued with noisome discharges—to the ordinary and larger wards, is a subject of dispute. Miss Nightingale remarks, that small wards for special cases can, in small hospitals, be attached to a pavilion:

> "But in large hospitals the smaller class of special wards should always be grouped together and completely separated from the other wards, because they are intended to contain either the most dangerous and important cases, or noisy cases, or cases with offensive discharges, which it is always safest to remove from the general wards; besides, small wards require, if possible, purer air than larger wards, and, therefore, more care in construction; and in order to insure those cases which really require most nursing from neglect, they should always be placed under a completely appointed staff of their own, and *not* attached, one to each large ward, which renders proper attendance extremely difficult." (Op. cit., p. 64.)

At a previous page of her work (p. 52) this experienced writer refers to small wards under the name of "casualty" wards, and declares that when constructed at the end of a large ward (as often seen in French hospitals), a room of this kind "is only an incubus."

Uytterhoeven, in like manner, proposes that special wards for patients after operation should be entirely cut off and detached from the ordinary wards; and Dr. Parkes lays it down as a rule, that "it is desirable to have the small wards not close to the large ones, but at some little distance" (op. cit., p. 297). Among the members of the Surgical Society of Paris much diversity of opinion prevailed as to the desirability of providing small wards at all for the purpose of isolation, but finally an affirmative conclusion was arrived at ('Discuss.,' pp. 127-129); and Larrey, in his able discourse, advocated that two separate small rooms (cabinets) should be annexed to every ward, to accommodate the most serious cases, as also patients after major operations, and, as a provisional and temporary measure, those suffering with contagious maladies. Messrs. Bristowe and Holmes are also in favour of small wards in connection with the main wards, and state the advantages of this position to be, that

> "The patient need not be taken from under the care of a nurse

to whom, perhaps, he has got attached; he is easily moved back again, if the necessity for his separation has passed away, and the convenience of supervision and nursing is much greater."

And in support of these views, they cite the experience of University College Hospital. They add—

> "These remarks apply to small chambers for one, or at the most two, separate beds. We can discover no utility in the plan which is met with in some Parisian hospitals, of dividing the main ward into two parts, one of which is about a fourth the size of the other." ('Report,' pp. 507, 508.)

M. Trélat, in his concluding speech ('Disc.,' p. 124), hit upon a most important point that must always materially influence the decision arrived at as to the mode of construction—the dimensions and the position of small wards for special cases. He contended that the isolation to be given is not alike in the case of a delirious patient and in that of one suffering with smallpox, or in that of one who has undergone the operation for cataract and in that of the victim of hospital gangrene. To cases of one sort a chamber adjoining a ward will suffice, to those of another a detached building is needed, whilst yet for those of other descriptions special structural arrangements are called for.

This being so, the recommendation of Miss Nightingale to group together the several special small wards of large hospitals appears to us to be open to serious objections, not counterbalanced by the advantages accruing from the supervision of a nurse especially assigned to the particular section thereby constituted. For it must be held objectionable to place noisy delirious cases in contiguity with patients recently brought from the operating theatre, or with such as are afflicted with hospital gangrene or erysipelas, or with any that can be called—in her own language—the most dangerous and important cases. The location of small special wards for the cases she has in view must therefore be determined by the precise character of those cases. For instance, the delirious and noisy should be disposed of in an apartment where they cannot prove annoying to others; the subjects of operations may be placed in the immediate neighbourhood of the operating room or of a surgical ward; whilst those who suffer with offensive noxious discharges need isolation in a room entirely cut off from the general wards. Lastly, sufferers from eye-disease will require the peculiar arrangements of an eye-ward.

The small place occupied by special wards in Miss Nightingale's treatise on hospital construction follows naturally as a consequence of her views regarding contagion and infection.

The former she treats as simply mythical, the latter as a phenomenon that ought to be unknown in a well planned and managed hospital.

> "Infection (she writes) and incapable management, or bad construction, are, in hospitals as well as in towns, convertible terms; . . . . (and) no stronger condemnation of any hospital or ward could be pronounced than the simple fact that any zymotic disease has originated in, or that such diseases have attacked other patients than those brought in with them."

Granting these premises, the logical sequence is, that distinct wards or hospitals for so-thought infectious diseases are superfluous; for such maladies "may, with proper sanitary precautions, . . . be treated in wards among other sick without any danger."

Neither the hypotheses respecting contagion and infection, nor the inferences deduced from them by Miss Nightingale, are accepted by the medical profession in general. There is an increasing conviction that cases of fever and of other contagious or infectious diseases are best treated in special buildings, where this course is practicable. Uytterhoeven would not admit cases of leprosy, of plague, of smallpox, or of cholera within hospitals situated in towns, but provide for them in separate buildings at some distance off in the open country. Years ago, he informs us, patients suffering with fever were mingled in the same wards in St. John's Hospital, Brussels, with others suffering from surgical injuries, and hospital gangrene then prevailed to a wide extent; but, after much opposition, the fever cases were isolated, and hospital gangrene disappeared from the wards, never to return, except for a while in 1830, when the hospital was greatly overcrowded with surgical cases. "The medical students have only learned what hospital gangrene is from books, for the wards have furnished them no opportunity for its clinical study" (op. cit., p. 50).

Husson quotes Tenon as advocating the isolation of contagious maladies, and states that the Commissioners of the Academy of Medicine, in their report, concurred in this recommendation. On the other hand, M. Blondel affirms that the balance of opinion in Paris is against the segregation of contagious diseases (op. cit., pp. 103, 104); and it would seem that Mr. Simon, together with Dr. Bristowe and Mr. Holmes, also entertains the persuasion that contagious diseases may be distributed in the general wards with impunity, provided that those wards are properly ventilated, that proper ward discipline exists, and that the number of fever cases does not surpass a certain proportion relatively to the whole number of inmates in a ward ('Report,' p. 64-70). This last condition Mr. Simon enforces by an appeal to the wide and telling experience of Mr. Whitfield at St. Thomas's Hospital, as recorded in the footnote at page 69 of his Report. Mr. Whitfield there shows that after medical and surgical patients, in 1828, were placed in separate wards, and

> "Whenever there was an epidemic of fever, the worst cases of fever were sure to get gathered together into particular wards. And now fever spread. The sisters, nurses, and pupils not only became victims to the condensation of the infection, but the mortality was great, so much so that the governors called upon the medical men to adopt some measures for its amendment. The plan then proposed was to limit the cases of fever in the old wards to four in each, and in the new wards (north wing), then recently built, to six. On the plan being carried out, it is but fair to say that with few exceptions it was attended with the most marked success; though an occasional case of typhus or scarlet fever has been communicated from patients to patients, or to the nurses in the hospital, it would be vain to deny."

This subject of the best way of dealing with fever patients being, however, one rather incidental only to the general subject of hospital construction under examination, must not further detain us.

In the matter of most of the accessory apartments of wards there is a happy uniformity of opinion among the several writers on hospital construction. Whether sitting-, day-, or dining-rooms separate from the wards should be provided, is, however, a debateable point. Miss Nightingale is adverse to such rooms. She makes the wards subserve all requirements. Her views are represented in the following extracts from her treatise:

> "Practically it will be found that all patients, as long as they need to remain in hospital at all, had better be in their own wards, under their own nurse, provided those who can go out have as much out-of-doors exercise as possible." (Op. cit., p. 114.)

It is practically impossible, she urges,

> "To keep such supervision over day-rooms as will prevent patients in them from playing tricks, and from doing themselves more harm than good. Recovery has been found to be actually retarded by days in the day-rooms. . . . It is very undesirable to mix the patients of the several wards when they have to return to them. They meet only for gossip; they prefer this gossip to out-of-door exercise. . . . The serious cases generally think the ward very dull without them, and call a large, full ward, 'so cheerful.' . . . Taking the general run of cases, when patients can go into day- and dining-rooms, they had better go out of hospital—not home, but into a convalescent institution."

An institution of this sort, particularly if quite away from the hospital, is, in her opinion, far preferable to convalescent wards, although these last are better than day-rooms for convalescent patients ;(pp. 114, 115).

These remarks apply only to civil hospitals; in military hospitals Miss Nightingale recognises the necessity of day- and dining-rooms and of convalescent wards.

The other writers on hospital construction do not share these opinions with the able authoress; on the contrary, they recognise the utility of day- and dining-rooms. The members of the Surgical Society of Paris were strongly impressed with the advantages accruing from the daily removal from the wards of such patients as could be transferred to accessory apartments; and Uytterhoeven enumerates several reasons for separating those capable of it from their fellow-sufferers in the wards, particularly during meal-time (op. cit., p. 108). Among the English authors, both Dr. Parkes and the reporters, Messrs. Bristowe and Holmes, may be cited on the same side; and those experienced in hospital management well know that there are many cases unfit to be sent out, even to a convalescent institution, but to whom a withdrawal from the companionship of their bedridden fellow-patients is both desirable and salutary. It seems beyond question that patients able to quit their beds can get their meals more satisfactorily in a separate dining-room than in a ward, and that they are more likely to enjoy their meals away from the sight of their more suffering companions and the many disagreeable impressions inseparable from a sick-ward. And surely it is an advantage to lessen for some hours or for most of the day the population of a ward, and to bring other moral influences, more attainable in a day-room than in a sick-ward, to bear upon the minds of those who can escape from the latter. The removal to a day-room must in itself be encouraging and gratifying to a patient; and certainly, unless day-rooms are most unfortunately placed, discipline may be maintained in them as well as in the wards. Respecting gossiping, is it, we may ask, always such an evil as to constitute a breach of discipline? This certainly cannot be averred; for gossip or talk must needs go on, more or less, among those collected together in the same apartment, unless the penitentiary system of silence be enforced, and hospital be assimilated to prison wards. Doubtless, indeed, hospital wards must be kept quiet; but is not this end to be attained more effectually and with less hardship by providing sitting-rooms for such patients as can leave them, and who will naturally desire to use their tongues?

The lesser importance given to baths in English than in French hospitals is remarkable.

"Strictly speaking (writes M. Blondel), no bathing establishment (*service de bains*) exists in English hospitals. Although in some institutions bath-rooms are casually placed in proximity to the wards, and in others two or three baths are arranged in a basement for the use of both in- and out-patients, such arrangements cannot be compared with those found in our hospitals. In these there are at least twelve to fifteen baths; a hot-air closet; a bath-room, with douches of all kinds, and hydrotherapeutic appliances at the command of the medical officers. To all these, moreover, are superadded movable baths for each ward, capable of being taken to the bedside of those seriously ill. In all our principal hospitals the bath-house (*salle de bains*), with its accessory furnaces and tanks, necessitates the construction of a special building. There are, indeed, some hospitals in which two distinct bath services exist. For instance, at the Hospital St. Louis there is one bath-house for out- and another for in-patients, each covering as much space as some London hospitals. Moreover, the number of baths given in this establishment have amounted to nearly 400 a day at a certain season." (P. 116.)

M. Blondel, in a previous page (p. 102), affirms as a general fact that baths are scarcely resorted to in English hospitals as means of treatment, but chiefly for the purpose of cleanliness; and he and his colleague, M. Ser, were much astonished at the practice prevailing at the London Fever Hospital, of giving a bath to every patient so soon as brought in, even before the examination of the case and its removal to the wards.

In adducing the example of the Hospital St. Louis as illustrative of the extent of bathing arrangements prevailing in Paris, M. Blondel has omitted to tell us that that hospital is exceptional, surpassing all others in Paris in that particular. Husson (p. 102) distinctly states this, and gives the details of the huge bath-houses, showing likewise that the one for outdoor applicants is simply a public bath for the use of the poor population in the midst of whom the hospital is located.

However, the neglect of baths as therapeutical measures in this country, as remarked upon by M. Blondel, is on all sides admitted, and within the last few years much has been done to remedy it. And as the importance of baths in the treatment of disease gets more fully recognised, the greater will the necessity be felt of erecting bath-houses in connection with hospitals, in addition to the bath-rooms as at present commonly appended to the wards. For without such separate special arrangement no facility for giving medicated baths and the so-called Turkish baths exists. Already, indeed, baths of the description last named have been added as appendages to some institutions in this country; and so encouraging are the results reported of their employment in disease, that they will ere long, without question, be generally adopted, and form a section of the "separate bathing establishment at a convenient distance from the pavilions, but connected by the corridors," which Miss Nightingale proposes as necessary to the completeness of every hospital of magnitude.

If the meagerness of the arrangements for baths in English hospitals struck the French Commissioners as remarkable, the furnishing of the wards at first perfectly astounded them. As soon as they entered the wards, they tell us, the nakedness of the walls and the almost poverty-stricken appearance of the furniture painfully impressed them. Judging from comparison (with Parisian hospitals), there seemed evidence of destitution, and the thought was suggested of the privations the patients were subjected to, particularly in a country where comfort is so much studied (op. cit., p. 67). However, they were in course of time somewhat re-assured on these particulars, and concluded that all this bareness of wards, though intolerable in Paris, might accord with the habits of the inmates. Besides, another effect was produced by it upon their minds, viz., that of making the wards appear to them much more spacious than they actually were, and of imparting generally an impression of greater cleanliness and freer ventilation; and their final general conclusion was, that

> "The English aim only at what is indispensable. They treat their sick patients without troubling themselves about means to render their stay in hospital less disagreeable, and without attempting to lessen the nauseousness of their medicines. On the other hand, they pay great attention to all details calculated to economise time and trouble." (Op. cit., p. 82.)

They are, moreover, fain to admit that the bareness of furniture in the wards (though probably a question rather of expense than of hygiène) opposes less obstacles than are found in French hospitals to the circulation of air, and offers fewer articles for impregnation by hospital miasms (op. cit., p. 83).

But although, on the whole, MM. Blondel and Ser found in this condition of English hospital wards little to recommend by way of example, it is, on the other hand, one which fulfils the rule laid down by Miss Nightingale, that "the less ward furniture, speaking generally, the better" (op. cit., p. 80).

The warmest dispute in the matter of ward furniture has been respecting the use of curtains. In few even among foreign hospitals do window-curtains survive; but curtains to bedsteads are still in rather extensive use both in France and other countries of the Continent, and to a much smaller extent in this country. Miss Nightingale dismisses the question of their use in a few sentences:

> "They are not necessary; they interrupt ventilation, and entail additional cost in washing. Where seclusion of a patient is required, a low, movable screen, not higher than the patient's head when he is sitting up in bed, is far preferable." (Op. cit., p. 79.)

The verdict of Dr. Bristowe and Mr. Holmes is, on the whole, adverse to their use. They regard them as presenting obstacles to ventilation, and in well-constructed and well-ventilated wards they are useless for purposes of health, except, perhaps, in some special cases:

> "Where the natural ventilation of the wards is good, where the curtains are not drawn during most of the day, and particularly where they do not surround the whole of the bed, no great harm appears to ensue; but where, as in the Parisian hospitals, all these conditions are reversed, we can hardly resist the conviction that they add to the unhealthiness of the wards and to the risks of erysipelas—a disease which is known to be so much fostered by bad ventilation, and so often to cling to beds where there is any source of local impurity. It is very possible, however, that in special cases, or in special hospitals (chiefly in those destined for affections of the chest), curtains, at least round the upper part of the bed, would be almost necessary, so that some beds capable of this addition, when required, ought to be provided." ('Report,' p. 498.)

The practice generally pursued in this country is gradually making its way in the hospitals of the Continent, in the face of much adverse opinion, and of prepossessions in favour of curtains of long standing. The comparative advantages and disadvantages of curtains were largely discussed in the sittings of the Surgical Society of Paris. M. Larrey would allow them only in wards for females, and then only on condition that they should never be drawn around the bed. M. Trélat would suppress them entirely, because they constitute surfaces for the collection of dust and impurities, and screens impeding the movements of the air; besides presenting obstacles to thorough supervision, and being capable of being used to conceal what ought not to occur, or not to exist. The advantages claimed on their behalf are of no practical importance when the positive disadvantages of their use are weighed in the balance against them ('Discuss.,' p. 124). MM. Le Fort and Verneuil coincided with M. Trélat; but the major part of the disputants inclined to the opinion that in exceptional cases, and in female wards mostly, curtains to the beds are desirable, and it was eventually decided that their presence or absence should be left to the discretion of each hospital officer.

Curtains to the beds are in general use in St. John's Hospital, Brussels; and Uytterhoeven defends it by the usual arguments. Husson is on the same side, and so also is M. Blondel. The last-named physician looks upon the narrow side-curtains of the beds in some few British hospitals as worse than useless, since they neither give protection from currents of air, nor serve to conceal any movements of the patient; and he is scandalised at the general absence of enveloping curtains, as indicating a disregard of decency, and as being in contradiction to the manners of the country. Along with other apologists for the retention of curtains, he contends for their utility against currents of air and strong light, and in diminution of the inconveniences and annoyances resulting from life in common. Open windows are most efficient in ventilating a ward; but how, asks the defender of bed-curtains, can you venture to open them unless you first enclose its inmates within the health-defending tabernacles of their beds? The reply is, that experience on the largest scale proves that this venture may be made without fear, provided the ward is well constructed and its ventilation good. But in one or other of these necessary conditions the wards in French hospitals are largely wanting, and the fear of mischief-working currents of air from open windows is often justified.

This reference to the conditions of ventilation brings us to the subject of warming and ventilating hospitals; but the length to which this article has already extended will forbid anything like an adequate examination of that important topic. Each of the works the titles of which appear at the head of this article is largely occupied with the discussion of the best means of ventilation. Ventilation implies a continual change of the air of the place, and consequently the movement of air:

> "The means by which air is set in motion are—1st. The forces continually acting in nature, and which produce what has been termed natural ventilation. 2nd. The forces set in action by man, and which produce the so-called artificial ventilation."—Parkes's *Hygiène*, p. 103.

Moreover, in some instances, natural ventilation is supplemented by artificial arrangements with the view of rendering it more complete.

The prevailing plan in this country is to trust the ventilation of hospital wards to the natural ventilation furnished by open windows, and by open fireplaces and doors. That it may be efficient, the windows must be placed opposite each other, and reach within a foot of the ceiling. They should likewise alternate with the beds (the bed-heads being placed within a short distance of the wall), so that each bed has a window on either side. This is a preferable arrangement; but in various hospitals two beds are allotted to the wall-space between every two windows, this intervening space being of sufficient width to allow the beds to be at least three feet apart. Dr. Bristowe and Mr. Holmes are justly adverse to this disposition of the beds in pairs between the ward windows, both as bringing them too closely together, and as having this other disadvantage—"that the interval between each pair of beds, where the morbid exhalations must be in the greatest quantity, is just that spot where the ventilation must be the most feeble." ('Report,' p. 497.) Besides, in their opinion, beds should not be less than six feet apart. The form of window is of material consequence: the French "croisée," opening vertically in the manner of a double door, is ill adapted for the natural ventilation of wards; whereas the English sash-window possesses much merit in this respect. It may be opened at top, an important matter, and its extent of opening above or below can be regulated at discretion, whilst a fresh inlet or outlet is furnished where the one sash slides over the other. This last ventilating aperture is made use of in some hospitals as the constant inlet for air, the lower sash being kept raised some six to ten inches, and a piece of wood, or better, a slip of plate-glass, placed below it, so as to close this lower open space through which the air might at times blow too directly and strongly into the ward. The form of window seen at the Middlesex Hospital obtains general approval. It is "divided into three parts, which open slopingly by a lever and pivot." Each division should, however, be constructed to open separately.

In certain states of the weather, and often at night, open windows are not compatible with the well-being of many patients; consequently, some means of ventilation have to be substituted for them. The principal of those proposed are, air-shafts from the ceilings of the wards to above the roof; air-holes through the walls near to the ceiling and also to the floor, and particularly behind the beds, in connection with out-cast ventilating shafts. To describe the many devices to make these apertures efficient, and to guard against draughts by "ventilators," is out of the question in this place. Those who seek information respecting such details may consult the works under notice, and the Parliamentary Report of the Barrack Commission, published some three years since.

The rule laid down by Mr. Simon ('Report,' p. 66) is under all circumstances to be observed, that, viz., "whatever other appliances exist, a ward must be ventilable by its windows;" and these, whenever the weather permits, should be more or less open.

The conclusion arrived at by the Surgical Society of Paris on the subject of ventilation agrees with the opinions of the English writers above expressed. It was held to be—

> "A mistake to suppose that a large cubic supply of air within the wards of an hospital can compensate for the want of air from without, or to think that plentiful artificial ventilation can fulfil the essential conditions of ward hygiène. Nothing can make up for the insufficiency or defect of natural aëration." ('Discuss.,' p. 135.)

M. Gosselin's testimony to the truth of this conclusion was unequivocal ('Discuss.,' pp. 88, 90). He stated that after reading M. Le Fort's remarks on English hospitals, he became fully impressed with the conviction that the greater salubrity of our institutions, as exhibited by the results of surgical operations, was mainly due to the free natural ventilation adopted, and he made up his mind to put this inference to the test. Wherefore, in 1862, when he was appointed to the surgical section of La Pitié Hospital, he carried his resolution into force. In the large ward of St. Louis, containing forty beds, there were nineteen windows, so constructed that small segments of them could be opened; in fact, however, the opening of these being left to the discretion of nurses and patients, it was only casually carried out. The consequence was, the constant existence of bad smells, the frequent prevalence of purulent infection, and such unsuccess of amputation, that the records of several years had to be searched to discover a successful amputation at the thigh.

The first thing M. Gosselin attempted, was to secure the constant open state of one or several of the large segments of the windows, except when visits were being made and dressings applied. Even during the night, and also in winter, one or more windows were kept partially open. For three years he persevered, and that dreadful bugbear of Frenchmen, cold and currents of cold air, became shorn of its terrors. Some colds befel the inmates, and one or two were attacked with slight pleurisy and pneumonia, but no proportionately greater number of such accidents occurred in these aërated wards than in others where the windows were kept carefully closed. But all inconveniences and accidents that could be possibly or in imagination charged against the proceeding were unworthy of consideration when viewed with the results recorded, viz., that unsuccess gave place to success in surgical operations, and hospital gangrene, along with other evils of ill-ventilated wards, abated or altogether disappeared. M. Gosselin appeals to statistics to prove his assertions, and further expresses himself convinced, that could the structural arrangements of the ward have permitted more complete natural ventilation, his success would have been even greater.

In opposition to the results of direct experience of this sort, the general objections, as urged by Blondel (really, too, upon supposition only) against open windows in wards for natural ventilation, scarcely demand notice. This writer cannot realise the existence of natural ventilation without the co-existence of injurious cold currents or draughts of air, or without undue depression of the temperature of wards in cold weather to the discomfort and detriment of the inmates. So he tells us that "the English sweep their wards by gusts of wind, whilst the French endeavour to purify theirs by gentle and unfelt currents of air;" and that, although he and two chilly compatriots well clothed, and with hats on their heads, were inconvenienced by the cold air, and complained of it, they were answered, much to their astonishment, that such inconveniences were much more tolerable than the want of pure air. Most Englishmen will endorse this view of the matter, though they will not sanction the "programme" M. Blondel derives from it—that we will have pure air, whatever be its temperature, and whatever may be its currents; for this programme implies indifference to conditions of the well-being of the sick, to which our countrymen will not plead guilty. It may be, however, that some, with Miss Nightingale, are prepared to accept absolutely M. Blondel's representation of English opinion in this matter, and to quote for his edification our Crimean experience as referred to in the 'Notes on Hospitals:' to wit, that, among other similar instances, "in the hospital tents of the Crimea, although the sick were almost without shelter, without blankets, without proper food or medicines, the mortality was not above one half what it was at Scutari." Moreover they might, indeed, quote Husson's history of the rough provision made for the multitude of wounded at the time of the invasion of Paris in 1815, in the then unfinished abattoirs of the city, and of the fact that the mortality in these ill-adapted places was less by one half than in the regularly organised hospitals, where cold and cold draughts were duly excluded.

Various are the systems of ventilation (combining also as a rule the means of warming the air of hospitals) that have been devised and proved by elaborate calculations and diagrams to possess every requisite to attain their object. But the lesson derived from experience of them is thus propounded by Mr. Simon:

> "Hitherto, I believe, without exception, plans of artificial ventilation for wards have been costly and fatal failures; and in the present

state of knowledge the utmost that can be said for them is, that some of their appliances (especially those for withdrawal of used air) may advantageously be used as additions to that more natural ventilation which, at least in this country, has appliances provided for it in the common conditions of decent house construction." ('Report,' p. 66.)

The majority of English hospital medical officers who have given attention to this question of ventilation concur in the opinions thus expressed by Mr. Simon, and an increasing number of French physicians do the same. In the course of the Discussion of the Surgical Society of Paris, MM. Giraldès, Le Fort, Larrey, and Trélat appeared as opponents to artificial plans of ventilation. The last-named termed them deceptive systems, and adduced the experience of French hospitals to prove his assertions. M. Uytterhoeven enters *con amore* into the subject of ventilation, and propounds a scheme of his own as the very best and cheapest, for the details of which we must refer the reader to his treatise on St. John's Hospital.

The artificial system of ventilation and warming resorted to at Guy's Hospital is one of several mentioned and unhesitatingly condemned by Dr. Bristowe and Mr. Holmes; but it is desirable to place here on record the results of the recent examination of that system made by Dr. Steele, the able superintendent of the hospital, and published by him in the last volume of the 'Guy's Hospital Reports' (third series, vol. xi, 1865).

The ventilating and warming of the hospital were entrusted to the late John Sylvester; and the general construction of the building was made, from the first, subordinate to the engineering requirements, and every facility given for carrying these out.

> "The design adopted appears to have been founded on a combination of the two systems of mechanical and natural ventilation, and to have comprised under the former an active extractive or appellant force, available in summer as well as in winter, and a less energetic means for the supply of fresh air by simply diminishing its density; a process which, of course, could only be effectually accomplished during the winter months. To realise these objects it was found necessary to make use of the entire space under the ground floor for the purposes of the engineer, as well as to erect the three great towers or air-shafts. . . It was originally intended, in winter at all events, that the windows should be kept closed, and that the interior should be indebted only to the adduction channels for its supply of fresh air; but this arrangement was found impracticable shortly after the wards were occupied, and as it has not been insisted on as a necessary part of the system, it would be unfair to regard the circumstance in the light of failure."

The openings for the exit of vitiated air were placed near the floor, and communicated by a series of flues with an extensive vitiated air-chamber, "which extends horizontally from one end of the building to the other, and terminates in the lofty smoke-shaft," within which the hot-water pipes and the ascending hot flues from the various fires in the wards were reckoned upon to increase the appellant action. "In addition, or rather in combination with the admission of fresh air to the wards, it was intended that the air so admitted should be" sufficiently and agreeably heated, even in the coldest weather, by an apparatus of hot-water pipes, and by open fireplaces in each ward. These last were to be used only exceptionally, the artificial supply of warm air being mainly, and as the general rule, relied upon. However, experience proved this to be impracticable, and after repeated trials the warm air introduced by the apparatus was abandoned as the principal means of warming the wards, and is now regarded as only auxiliary. A deficiency of heating power was experienced in very cold weather:

> "But a stronger objection than this, and one which is applicable to all artificial systems of heating, arose from the want of any controlling element to moderate the continually recurring changes in the external atmosphere—a want that can only be supplied by an intelligent use of the open fireplace. A sudden increase in the temperature outside was generally found to produce that stuffy sensation so familiar to every one accustomed to rooms overheated by artificial means. It is true that the effect might be obviated partially by opening the windows; but this is not always practicable during cold weather, nor could it be kept up continually in a moderately cold atmosphere. In fact, the system of heating by hot air alone interferes most materially with that ready and frequent change of atmosphere which is so indispensable to the healthy condition of the hospital ward, and no substitute has yet been introduced for the open fireplace which can in any way equal it in efficiency, either as a means of distributing heat or of contributing to the agreeable atmosphere of the ward."

These are important admissions from one who might be supposed to entertain some feeling of favour towards a plan of ventilating and warming elaborated in the hospital with which he is connected, without regard to cost and with every facility afforded to render it successful.

> "Since 1858, in consequence of the close and stuffy condition of the wards (to continue to quote Dr. Steele's statements), which was the cause of frequent complaints, large ventilating openings were introduced, opening into the wards from the staircase, and lowered panes of glass were placed in every alternate window."

These arrangements have proved very beneficial to the ventilation of the wards; but, as Dr. Steele proceeds to remark, it is barely possible, in cold weather, to obtain a temperature of 60° in a ward, and at the same time provide for such a renewal of fresh air as to keep the ward free from hospital smell.

> "It is always found that, in proportion to the increase of artificial temperature, organic impurities become more obnoxious, and the necessity for a renewal of the atmosphere becomes urgent, at a time when it would be unsafe to renew it directly by means of open windows. In other words, in cold weather a moderately cold atmosphere is more compatible with a healthy condition of a ward, than one raised by artificial means to 60 or above 60 degrees."

The fresh air for the wards of the new building at Guy's Hospital was arranged to be taken from a high level above the roof, and to be drawn "by aspiration" through the octagonal turret, becoming warmed in winter by traversing along the channels containing the heating tubes. It is found, however, that in summer, when no strong winds prevail, the fresh air becomes so impeded in the intricate passages it is made to follow, that it seldom contrives to make its entrance perceptible in the wards, whilst "the action of the separate flues when assisted by heat is exceedingly eccentric and various, some acting with great energy and uniformity, others with only a perceptible current, while a few . . . appear to have lost their action entirely."

On the other hand, the extracting power exerted through the flues, the apertures of which are placed near the floors, appears to be for the most part constant and energetic both in summer and winter, and whether the windows are open or closed.

With reference to the position of these extracting or eduction orifices near the floor, Dr. Steele makes the following important observations:

> "It has been noticed that the foul air currents on a level with the floor were usually from three to six degrees lower than the general ward temperature and the temperature of the incoming warm air. This fact of itself proves that the warm atmosphere is not so perfectly diffused throughout the apartment as the supporters of the system would lead us to infer. With regard to the other objects sought to be attained by placing the extraction orifices near the floor, they also appear more or less objectionable, or, at all events, open to question, when applied to the ventilation of an hospital ward. The main arguments in their favour rest upon the expediency of removing the foul air at or near the spot where it is generated before it has time to mingle with the air of the room, and on its permitting the fresh air to be introduced from a higher stratum than is usually adopted to diffuse itself universally, and to aid the chimney ventilation. The system appears certainly well adapted for rooms liable to become crowded and overheated by gas, and has latterly achieved a

signal success in the new theatres at Paris, where the heat of the gas-lustres is utilised to produce a powerful appellant action. But in the wards of hospitals in which a large amount of space is furnished to each patient, and where a system of ventilation unimpeded by variation in temperature or by the force and direction of the wind-current is in use, a ready exit to the foul air by apertures near the ceiling has never been found to act unsatisfactorily. It is difficult to understand also how the respired air issuing from the lungs at a temperature of 98° Fahr. will not prove more obnoxious, ascending by virtue of its own levity, and afterwards meeting with resistance from the incoming air, by becoming diffused through the apartment before it is removed by the orifices of evacuation."

The conclusions arrived at by Dr. Steele are, that the extraction power of the apparatus is energetic and useful; that, as a means of ventilation by the admission of air, the apparatus is valueless during summer, though in winter it contributes materially to ventilation; and, lastly, "that the current cost of providing fuel for the apparatus is not greater than is required for other methods of hospital warming, and is small in comparison with the advantages obtained" in the agreeable temperature in the wards during severe weather.

With this abstract of Dr. Steele's valuable contribution we must bring our remarks to a close. Most of the chief questions relative to the ventilation and warming of hospitals are dealt with in it; but those of our readers who may desire further information on those subjects may read with advantage Miss Nightingale's short and pithy observations; Dr. Parkes's able chapter in his standard treatise on Hygiène; Husson's epitome of French official reports, and the chapters by Blondel and Ser; those of the latter especially.

We had sketched in the programme of this article an outline for treating of the subject of hospital nursing, the importance of which is daily more and more recognised, but to carry out the design would expand this paper beyond reasonable limits.

Review VIII.

Transactions of the Obstetrical Society, Vol. VII, 1866; *with Classified Catalogue of the Library.*

This volume contains communications on monstrosities, extra-uterine fœtation, uterine tumours, procidentia uteri, ovariotomy, operations on the uterus, dermoid cysts, imperforate bowel, complicated pregnancies, &c. &c. Mr. Wills, of Totness, Mr. Gayton, and Dr. Meadows, relate each a case of monstrosity, where the abdominal walls were absent, and the development of the viscera was more or less arrested. Mr. H. Hailey gives a case of acrania, being twin with a perfect fœtus. There was only one placenta. Mr. Waits and Dr. Tanner relate each a case of hernia cerebri; Mr. Ellis, of Newcastle, a case of cyclopia occurring in twins.

Three cases of extra-uterine fœtation.

Case i, reported by Dr. Playfair.—Impregnation took place in February, 1862; an attack of abdominal pain and lipothymia occurred in April, but nothing further except some abdominal pain and vomiting in July. In November a profuse bloody discharge continued for thirteen weeks. In February, 1863, great constitutional disturbance came on, with severe pain in the back and abdomen; extra-uterine fœtation was diagnosed, and the question of operation raised, but negatived. Later, an attempt to open the abdomen and cyst by potassa fusa was begun, but the patient gradually sank in September.

Autopsy.—A cyst was found containing a mass of adipocere, in which were imbedded the bones of a nine months' fœtus. Only a small portion of the left Fallopian tube could be traced, the remainder, with the ovary, being lost in the cyst and surrounding adhesions.

Case ii, reported by Dr. Braxton Hicks.—Metrorrhagia drew his attention to the probability of extra-uterine fœtation. The recto-vaginal peritoneal pouch was filled by a live fœtus about three and a half months old. The uterus was enlarged. Unsuccessful attempts were made to stop the growth of the fœtus by galvanism; afterwards a trocar was passed through the vaginal wall into the fœtus, but no blood followed; this was under chloroform, and vomiting followed for three days. Tenderness and fulness were perceived in the inguinal region, and on the fifth day symptoms of internal hæmorrhage, with collapse and death, occurred.

Autopsy.—The uterus was found enlarged; a cyst was adherent to its left side, and fundus distended with purulent serum. The cavity was lined with flaky lymph and filled with the fœtus, whose embryotic membranes bulged into the cyst. The placenta was adherent to the posterior surface of the uterus and the right side, as much as was not occupied by the cyst.

Case iii, reported by Mr. Truman, of Nottingham.—A multipara, whose last child was seven years old. The menses (?) appeared on March 10th, 1863, lasting only two days instead of the usual four, and she complained of pain and tenderness in the right iliac fossa; pyrexia continued until June 6th. On June 7th she stood on a chair to take down a picture, having to stretch a little for that purpose; she turned deadly pale, and complained of a queer sensation in her bowels, lay down, and continued in a state of syncope until her death, in two hours' time.

Autopsy.—No tumour of any kind felt by vaginal and abdominal examination; cervix uteri shortened; os soft, patulous. On opening the abdomen a quantity of sanguineous serum escaped; a coagulum of blood filled the true and false pelvis. The uterus, enlarged, was pushed to the left side by a mass situated posteriorly to it, about the size of a large orange. On its upper surface was seen the opening of a vein, from which blood could be made to issue; this mass was adherent by recently formed adhesions to the pelvis. The tumour was lined with a placenta; a section at the upper part showed, externally, a corpus luteum, around which was a pinkish stroma; internal to this was a thin layer of a fibrous character; and next, and between this layer and the placenta, a mass of blood-coagulum.

Dr. Barnes considered the menorrhagia in these cases was caused by the growth of the placental villi being more rapid than that of the sac, and compared it with placenta prævia.

Four cases of ovariotomy are reported, with discussions on the different modes of treating the pedicle.

Mr. B. Brown advocated the division of the pedicle by the actual cautery, and then returning it at once into the abdomen.

In the other cases the pedicle was fastened—(1) by a clamp (Mr. S. Wells); (2) by a silk ligature, whose ends were brought through the abdominal incision and wound round a hairlip-pin (Mr. Sharpin); (3) by a double-thread ligature, the four ends being drawn through two iron-wire coils and secured to a cross bar outside the abdomen (Mr. Aveling). The last two and Mr. B. Brown's procedure are adapted to cases where the pedicle is short. Mr. Marion Sims observed that the ligature, returned into the abdomen, whether metallic or other, became sacculated, and quoted several autopsies where no sloughing of the pedicle so treated had been found.

Mr. Albert Napper relates a successful case of amputation of the arm, the patient being in the eighth month of pregnancy.

Dr. Braxton Hicks, in a paper "On the Mechanism of Delivery of Face Presentation in the Mento-posterior Position," argues that, on comparing the diameters of the outlet of the pelvis with those of the head, there is no reason why, at times, provided the chin cannot rotate from behind forwards, the head should not be delivered in the mento-posterior position. He shows that this condition has either been passed over or imperfectly described by nearly all the authors on obstetrics, but Professor Braun, in a communication "On a Rare Mechanism in Face Presentations" ('Monatsschrift für Geburtskunde,' 1861), describes two cases, in one of which he delivered by the forceps; the root of the nose first became visible, the chin passed over the perinæum, and then the calvarium and occiput came under the symphysis pubis in completely transposed mechanism. In the other case the child was born naturally, but was dead. Dr. B. Hicks relates two cases in his own practice:

Case i.—A multipara, æt. 40. The face presented, the forehead anterior and rather lower down than the chin, which was pointing to the sacrum. After an ineffectual attempt with the forceps to improve the position of the head, so as to make the vertex descend first, I with my hands produced slight rotation of the chin to the left side of the sacrum, then reapplying the forceps drew down the chin over the sacrum and the perinæum; immediately upon this the upper part of the head glid underneath the pubic arch, and the delivery was quickly over. The vertex was much depressed, appearing rather as a hollow, and the head was wonderfully elongated backwards on to the neck. The pelvis was full-sized, the child rather larger than the average of males.

Case ii.—A primipara, æt. 19. I found the chin rather towards the right side of the sacrum, the forehead forward. The pains having gone off, secale was given and they returned; I failed in an endeavour to bring the chin anterior by the hand, but it descended, and shortly the forehead separated the vulva beneath the pubic arch, the face and chin gliding down easily over the sacrum and inside of perinæum till the nose was just clearing the anterior margin; then after a slight detention the chin rotated forward beneath the tuberosity of the right ischium up into the arch to the position in which it makes its exit in the more usual cases. I have little doubt but that the face would have escaped by natural powers into the mento-posterior position had there been more difficulty in its rotation forwards. Pelvis was normal. Child quite an average male. I may sum up our knowledge on the subject in the following words. That although in face presentations the form of the normal pelvis and of the fœtal head are such as in the majority of cases to cause the chin to rotate beneath the symphysis pubis from whatever position it originally held, yet that in some cases this rotation is deferred till the face is partly appearing externally; that in some rare cases the rotation may be incomplete, the face being delivered in the oblique mento-posterior position; that in some very rare cases the chin, unable to pass forward at all, the face is delivered by the natural efforts alone, the chin sweeping over the perinæum, while the vertex, much compressed, passes out beneath the pubic arch; but that in the greater number of these rare cases the natural powers are insufficient to expel the head, which, unable either to rotate or advance, requires artificial aid for its delivery; that in this case the face may be born either with the chin appearing first or the forehead or vertex.

In practice, if the head descends readily, we may wait till it is arrested, and if the hand cannot direct the chin forward we must with the forceps first endeavour to rotate the chin forward, and, failing that, to draw the head down in that manner which will be found most practicable, that is, with either the forehead and vertex first or the chin.

Dr. Robert Barnes communicates a paper "On the Varieties of Form imparted to the Fœtal Head by the Various Modes of Birth."

The measurements are obtained by laying the head of the new-born child on a sheet of paper and tracing its profile, and then laying the occiput on the paper and tracing the greatest circumference, verifying the lines by applying the calipers to the head. Dr. Barnes concludes that—

> "Under all modes of birth, if the head is even a little delayed in its transit through the brim, it will be unequally moulded on two sides. The side which is squeezed against the promontory of the sacrum will be always somewhat flattened, whilst the side which is directed towards the symphysis pubis, suffering generally less compression, preserves more of its normal rotundity. The deformation is threefold—1. Elongation or conification. 2. A symmetrical flattening of one side. 3. Twisting the conified portion upon its axis."

The form impressed upon the head at birth is often to a considerable degree retained through after-life. The cases with illustrative figures exhibit—1st. The natural or standard form and size of the fœtal head at term. 2nd. The forms imparted to the head under turning in contracted pelvis. 3rd. A head born after turning, the brim being contracted and the face having presented. 4th. A head born after craniotomy. 5th. The forms acquired under tedious labour terminated by the forceps.

As the details of the fourteen cases reported will not bear abridgment, and require the diagrams, we must pass them over and quote the deductions drawn by Dr. Barnes:—1. The ordinary dimensions of a standard head at term, not deformed by labour, are—

Fronto-occipital diameter	4·50 to 5·00
Occipito-mental diameter	5·25 „ 5·50
Greatest transverse diameter (between parietal protuberances)	3·75 „ 4·00
Lesser transverse diameter (between the ears)	3·50

2. In protracted labour, with vertex presentation in a normal pelvis, the above dimensions altered to—

Fronto-occipital diameter	5·25 to 6·00
Occipito-mental „	6·50 „ 6·75

The greatest transverse diameter is often merged in the lesser—that is, the parietal bones are compressed so that the inter-parietal diameter becomes the same as the inter-auricular, which is fixed.

3. In a case of tedious labour, with the head in Naegili's third position, the elongation was greatest in the bregmato-mental diameter.

4. Elongation of the head is due sometimes entirely, and often greatly, to the pressure the head experiences in passing a rigid, imperfectly dilated cervix uteri.

5. In turning in contracted pelvis from projecting promontory the transverse flattening of the head is much exaggerated; in extreme cases space is gained by indentation or fracture of the bone in contact with the promontory; the lateral or transverse compression is compensated by slight mento-occipital elongation, and also by fronto-occipital elongation.

Three cases of obstructed labour are related by Professor Barry, of Birmingham, and one by Mr. Roper. In the first case the difficulty arose from distension of the abdomen of the child by a cyst formed by the continued growth of the allantois, which communicated with the bladder. Puncture of the abdomen relieved and allowed delivery. In the second case a dropsical ovary filled the pelvis. Delivery was effected with the forceps, but the vagina ruptured and the ovarian tumour escaped through the rent after delivery; it was removed by ligature and knife. In the third encephaloid disease of the sacrum formed a tumour, obstructing the birth. Delivery was effected with great difficulty by turning and perforating, but death followed in an hour, and the uterus was found to have ruptured. In Mr. Roper's case the difficulty was due to "a non-evoluted and hypertrophied state of the cervix uteri." The os externum admitted the hand, but not so the os internum until thirty-six hours after the liquor amnii had escaped. Delivery was accomplished by craniotomy and the long curved forceps, but death from pyæmia resulted fifteen days later.

Dr. Greenhalgh contributes two papers on Cæsarean section in cases of extreme pelvic distortion, contrasting this operation with that of craniotomy. In the first paper a case is detailed of a woman aged twenty-eight, in labour with her second child. The first labour, in the year previous, had lasted fifty-four hours, and was terminated by craniotomy, with separation of the head from the body; a vesico-vaginal fistula also resulted. She was now in the eighth month of pregnancy, and labour had come on spontaneously. On examination the os was fully dilated; the left foot presented at the brim, of which the antero-posterior diameter was an inch and three quarters. Dr. Greenhalgh drew down the feet, opened the head and crushed it; after pulling for twenty minutes the head separated from the body and collapse came on, when she rallied. Cæsarean section was performed, the uterus was opened, but the head had escaped, and was found under the diaphragm; the placenta was removed. Death ensued in thirty-one hours. At the post-mortem a rent was found in the uterus, running upward and backward from the vesico-vaginal fistula. No union had taken place either in the uterus or the abdominal walls.

In the second paper Dr. Greenhalgh criticises the axiom "that meddlesome midwifery is bad." "I am confident," says he, "that far fewer evils result from too early interference than from too great delay; *laissez-faire* midwifery is bad."

He relates a case of Cæsarean section, at which he was assisted by Drs. Meadows and Eastlake. The patient began to lose health after her fifth labour, which lasted thirty-six hours, and the child was dead, the previous four having been normal. She was now at term; labour had been on nearly five days; there was extreme distortion of the pelvis from mollities ossium, with presentation of the right tuber ischii at the brim. It was decided that delivery per vias naturales was an impossibility, and Cæsarean section was performed. Death from exhaustion followed in eighty hours.

Autopsy.—The abdominal wound had nearly all united; the cavity was free from blood, pus, or serum. The uterus was contracted, its wound gaping for about an inch.

Dr. Greenhalgh then proceeds to notice some fatal cases of craniotomy in the practices of Dr. Murphy, Dr. Lee, and himself, death usually resulting from rupture of the uterus, and argues "that craniotomy and crotchet operations are only safe within certain limits, and that cases every now and then occur in which Cæsarean section would be attended with no greater hazard to the mother. Nothing," he says, "would induce me again to attempt delivery by the crotchet when the conjugate diameter of the brim does not fully measure two inches exclusive of the soft parts."

In the discussion that followed the reading of the paper

Dr. Barnes said he believed that in the most extreme degrees of contraction it was possible to deliver by craniotomy at seven months, and that two inches' conjugate diameter was sufficient for a full-grown child.

Dr. Playfair thought that Cæsarean section was especially applicable in cases where an ovarian tumour was pushed down in front of the fœtal head and obstructed delivery. Dr. West calls attention to "an abnormal variety of battledore placenta." In a case which he met with of twins having only one placenta, which had no raphé of cohesion, and, consequently, the fœtuses were enclosed in one bag of membranes, the funis of one was inserted normally in the centre, but that of the other divided and branched for some distance before juncture with the edge of the placenta. Dr. West remarks that, although the funis of one child had been torn from the edge of the placenta, the other child had not suffered from hæmorrhage.

Dr. Rasch brings forward a case of œdema of the lower half of the body after a fall in the seventh month of pregnancy. The urine was highly albuminous, but there were no casts. Premature labour was induced, and the woman perfectly recovered.

Dr. Bathurst Woodman details a case of chorea connected with pregnancy. Dr. Graily Hewitt relates a case of antiflexion of the gravid uterus, so marked that it did not rise out of the pelvis for some days, and caused difficult micturition; it was relieved spontaneously.

Four cases by Drs. Woodman, Barnes, and Russell, are detailed of cystic disease of the chorion, three of which were complicated with albuminuria, and two ended fatally. Dr. G. Hewitt is of opinion that—

> "In cases of albuminuria, abortions are liable to occur in consequence of imperfect nutrition of the fœtus. The fœtus dies, and as a result (the ovum remaining still within the uterus) the chorion villi undergo the hydatidiform degeneration."

Dr. Bannister sent to the society a fœtus between seven and eight months, of which a patient had been delivered spontaneously. Dr. Harley and Dr. Meadows reported that there was a rent four inches in length across the abdomen, exposing the upper surface of the liver, which had all the appearance of having been inflicted with a cutting instrument, and another wound two and a quarter inches across the throat. The funis had been torn off close to the body. There was no properly formed bony tissue in the skull. The viscera were all natural. The skin was both brittle and exceedingly thin; microscopically, hardly any yellow elastic and very little white connective fibrous tissue could be discovered; to this arrest of development was due the feeble resistance of the skin. The birth was probably abdominal. Under the skin there was little or no areolar or connective tissue, but only bundles of fat-vesicles heaped together. Under this adipose layer was a perfectly smooth glistening fascia.

Dr. Meadows relates a case of imperforate bowel, with an illustration. He exhibited a perforation of the rectum of an infant, which terminated blindly at an inch and a quarter from the anus, which was perfect, and ended in a cul-de-sac. Two futile attempts were made to reach the bowel. The child died twenty-two days after birth. Dr. Meadows suggests that a later attempt, when the bowel was more dilated, might, perhaps, have been successful.

Mr. Fred. Lawton relates a case of vascular (erectile) tumour in the sheath and at the base of the funis of a new-born child. The opening of the abdominal walls allowed umbilical hernia. The tumour was ligatured and cut off, the hernia reduced, and four needles with pad and bandage finished the operation. The child did well.

Three instances of "dermoïd cysts" are reported, one by Dr. Tyler Smith, which was passed per rectum with teeth attached. One by Dr. Woodman, attached to the right ovary, which was itself converted into a polycystic tumour. Two rudimentary teeth and hairs grew from the inside. A third by the late Dr. Ritchie. This was developed in the Fallopian tube; it was as large as a plum, contained four loculi filled with a creamy fluid, and a plate of true bone.

Puerperal fever is the subject of a paper by Dr. Snow Beck; the chief points are that the inflammatory actions and products are mere accidents occurring in the course of the disease, which is consequent on the introduction of a poison into the system, such as the secretion from the uterus, which enters by the sinuses, which are pervious in an uncontracted state of the uterus. The rationale of treatment is therefore to prevent further impregnation, by causing contraction of the uterus and coagulation of blood in the uterine sinuses. The injection of the sulphites or of perchloride of iron, &c., would act antiseptically, while the internal administration of stimulants and sulphites would fulfil the indications of general treatment.

Drs. Oldham, Barnes, B. Hicks, and Greenhalgh, detail more or less fully operations for the removal of uterine polypi and fibrous tumours. Mr. Harris gives an account of a uterine deciduous membrane shed shortly before labour came on, which Dr. G. Hewitt and Dr. Meadows supposed to be the lining of one of the divisions of a bifid uterus, the ovum of which had escaped in the first months of pregnancy, while the other division had carried its ovum to "the term."

Dr. Woodman describes a chancre on the os uteri, found at the post-mortem of a woman who had been admitted for ulceration of tonsils and pharynx, with anæmia; no history or other signs of syphilis. The child, four months old, was quite healthy.

Dr. R. Barnes, the president of the society, in a paper "On Dysmenorrhœa, Metrorrhagia, Ovaritis, and Sterility," considers these phenomena in their connection with the abnormal projection of the cervix into the vagina, and being of a conical shape. The obstruction, by its shape and rigidity, to the menstrual flow at each "period," reacts upon the ovaries; and all the phenomena of obstruction, dysmenorrhœa, congestion, hæmorrhage, hæmatocele, ovaritis, pelvic peritonitis, may follow. "Sterility is almost a constant condition." The principal means of overcoming the coarctation of the os externum uteri are dilatation and incision. The latter possesses a marked superiority in its results. Dr. Barnes then notices various instruments. He describes the one he uses as a kind of scissors, one blade probe-shaped, the outside blade being curved.

The results, he affirms, are extremely satisfactory. The congestion, inflammation, ovaritis, menorrhagia, and pain, are relieved. In the discussion that followed, Mr. B. Brown coincided as to treatment, but differed as to the seat of stricture, which he believed to belong to the whole cervix, and not chiefly to the os externum. Dr. Greenhalgh was convinced that in the great majority of cases the seat of stricture is at the os internum, and that the cervix is enlarged and not tapering, therefore division of the os internum was essential. Dr. Routh agreed with Dr. Greenhalgh as to the seat of stricture and mode of operation, and the necessity of keeping the cut patent by an internal uterine pessary.

Dr. Savage believed that obstruction at the isthmus was generally caused by uterine curvature, that the dysmenorrhœa dependent on congestion could be as well relieved by leeches as by the hysterotome, and that a judicious and persevering employment of the sponge tent was quite as efficacious as the surgical cutting. Dr. Wynn Williams thought that if more study was devoted to therapeutics cases requiring surgical interference would be reduced to a minimum, and instanced a patient who, suffering from gouty inflammation, had been operated on without benefit, but was relieved by remedies to counteract the constitutional tendency—rest and leeches. When incision was required, only so much of the tissue should be divided as was absolutely necessary, and a tent might be necessary for a time. Dr. Marion Sims, at considerable length, and referring to statistics of his own note-books on dysmenorrhœa, agreed with Dr. Barnes in the pathology and treatment.

Dr. Marion Sims contributes a paper on chronic inversion of the uterus, and details two cases, one of nine months' standing, in which he amputated the organ with an écraseur, and successfully overcame the hæmorrhage which occurred during the operation; and in the other, of twelve months' standing, he successfully reduced the uterus by taxis.

A second paper "On Procidentia Uteri" recommends M. Huguier's operation when the subvaginal portion is elongated. Dr. M. Sims is wrong in asserting that the French surgeon operated in every case. M. Huguier carefully discriminates, and only recommends ablation of the cervix, especially when the supra-vaginal portion is elongated, as a last resource, when pessaries cannot be worn. When there is no elongation Dr. M. Sims says that the procidentia begins by prolapse of the anterior wall of the vagina, and that in these cases the remedy is to remove a portion of the redundant tissue, and thus to narrow this wall, as Mr. B. Brown and others, beginning with Dr. Marshall Hall, have recommended the narrowing of the posterior wall, and the making a new perinæum when it has been ruptured. Dr. Sims gives illustrative diagrams of his mode of operation.

Review IX.

A Practical Treatise on the Diseases of the Testis and of the Spermatic Cord and Scrotum. By T. B. Curling, F.R.S., Surgeon to the London Hospital, &c. Third Edition, revised and enlarged. *London,* Churchill, 1866. Pp. 609.

Few surgical works have attained a more solid or a more deserved success than the one which we have now under notice. Mr. Curling's work has, in fact, become the classical authority on the subject of which it treats, as indisputably as 'Lawrence on Hernia,' 'Cooper on Dislocations,' 'Brodie on the Joints,' or any other of our familiar text-books; and it now enjoys the advantage, which none of the great works mentioned above has as yet received, of being brought down to the very latest condition of surgical knowledge, and enriched by the experience of foreign as well as English surgeons.

It would be completely out of place here to speak in terms of praise, however well deserved, of a work like this, which every surgeon knows and constantly uses when in doubt or difficulty. Our humbler task will be to explain to our readers how far Mr. Curling has been able to extend our knowledge of this important class of diseases, and in what his present edition differs from that which was reviewed on its first appearance in the seventeenth volume of this Review (p. 55). In doing so we shall follow the arrangement of that notice.

In the first place, that which formed the first part of Mr. Curling's original work was a treatise on the anatomy and physiology of the organs, which has been now omitted.

The first section of the work, as it now stands, treats of the congenital imperfections and malformations. Little change has been made in the first two sections of this chapter, treating of *numerical excesses and defects of the testicles,* and of *deficiency and imperfections of the vas deferens,* though a careful comparison will show that several interesting and instructive examples, mainly from foreign sources, have been added. In the next section, on *imperfect transition of the testicle,* has been incorporated the most valuable portion of the first part of Mr. Curling's original edition, viz., his ingenious explanation of the normal process and of the functions of the gubernaculum. The author's views, both on the normal process and on the cause of its occasional failure, remain unchanged. An important change has, however, taken place in his opinions with respect to the condition of the testicle when undescended. In the first edition, after stating John Hunter's opinion, that "when one or both testicles remain through life in the body, they are exceedingly imperfect, and probably incapable of performing their natural functions," Mr. Curling ranged himself on the side of Professor Owen, who maintains the contrary. The subject is one of some importance, since (as we stated when reviewing the first edition) doubts of their virility have more than once led the unfortunate victims of this malformation to suicide. Mr. Curling's former opinion, that in such cases virility might be unimpaired, was given with an amount of hesitation which we remarked upon (p. 59), and which we are sorry to say is justified in this edition by the relation of several cases in which the secretion of such glands has been found to be destitute of its characteristic element—the spermatozoa. We will quote the author's statement on this point:

> "When the testicle has not passed into the scrotum the gland is nearly always small in size; generally it is healthy, but undeveloped—that is to say, it has not undergone the enlargement and change in structure which take place at puberty. In some instances, especially when seated in the inguinal canal, it is withered and atrophied, having suffered fibrous and more rarely fatty degeneration, and ex-

hibiting no trace of glandular structure. . . . There are many well-authenticated cases of cryptorchics who, like Hunter's case, had a masculine development, sexual desires, and powers of copulation. Nevertheless, recent investigations show almost conclusively that a retained testicle only imperfectly executes its functions, and is incapable of forming healthy fertile semen. . . . Godard, in a memoir read at the Société de Biologie, mentioned the cases of three cryptorchic married men who had no children, and affirmed that such persons were always sterile. This earnest and indefatigable pathologist, in a more recent work, supported this opinion by additional facts. As considerable doubt existed of the soundness of this view I took some pains in examining the question, and made known the results in a memoir published in April, 1864.[1] In this paper I collected nine cases in which the ejaculated semen of men with retained testicles, or with a single detained testicle (the other having been removed or its excretory duct obstructed) was destitute of spermatozoa." (Pp. 27, 28.)

In cases of retained testicle, whether complicated with hernia or not, Mr. Curling recommends the application of a truss; and in this he is, we believe, supported by the opinion of the majority of surgeons; since not only is the occurrence of hernia with all its dangers prevented, but the testicle itself is kept in a situation less exposed to injury than if it came into the groin. As to the feasibility of transferring by operation a detained testicle from the groin into the scrotum, Mr. Curling gives two cases in which such an operation has been attempted, and in one, at any rate, with apparent success, but in general he believes that the attempt will be futile; and entertaining the views which he does as to the condition of the gland, he necessarily also thinks that success would produce no real benefit. In operations, therefore, for hernia where a retained testicle is met with, he advocates the removal of the gland; and the same course should be followed when the detained testicle is subject to attacks of pain and inflammation which cannot be remedied by mechanical treatment. The diagnosis of retained testicle having been laid down, Mr. Curling passes on to other mal-positions of the testis. He gives a good representation of a case of testicle situated in the perinæum, with numerous instances of this malposition, which he proposes to remedy by an operation in early life. This he had once an opportunity of doing in infancy, but the case remained incomplete, owing to the death of the infant from an accidental cause, unconnected with the operation. Another still rarer anomaly is noticed, in which the testicle passes into the thigh in the course of a femoral hernia. The treatment consists in reducing it into the belly, and applying a truss. A more frequent deviation is the inversion of the gland, so that the epididymis is situated in front. This has been commonly recognised as a cause of abnormal position of the testicle in hydrocele. Reversion of the testicle has also been noticed, the upper end being directed downwards, but it has no practical importance.

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[1] In this Review.

On *atrophy*, which is the subject of the next chapter, Mr. Curling contributes merely such additional information as will be found interspersed throughout the whole work, without, however, adducing any absolutely new opinions. Nor need the next chapter, on *injuries*, detain us. Mr. Curling alludes to the cases which Mr. Birkett has published, of supposed rupture of the vas deferens without external injury, but does not quite admit the certainty of the diagnosis.

On *hydrocele* (chap. iv), Mr. Curling's work must necessarily supply us with much that is valuable and interesting. We are surprised, in the first place, at the extreme frequency which Mr. Curling attributes to inflammation of the tunica vaginalis. Such inflammation is, says he—

> "Not only the most frequent disease of the testicle, but also one of the most common affections to which the body is liable. . . . Adhesions between its opposed surfaces are scarcely less common than those of the pleura. In examining the testicles of twenty-four adults I found fibrinous adhesions of greater or less extent in one or both glands in nine instances."

We confess that our own researches have left a different impression upon our mind; but we must allow that we have preserved no notes, although it was at one time our habit to examine the testicles in every post-mortem examination that we performed, and in those days this was no trifling number. The point, however, is of little importance. Inflammation of the tunica vaginalis, after its conversion into a hydrocele, is also spoken of by Mr. Curling as a common cause of the loss of transparency and the thickening of the sac. This inflammation is commonly chronic, and, when acute, is the result in most cases of injury; but Mr. Curling gives (on the authority of M. Gosselin, his French translator) a reference to a well-marked case of spontaneous acute inflammation of a hydrocele.

With regard to the symptoms of hydrocele, although the author's description is, of course, both lucid and complete, we do not think it necessary to extract anything except the following, which will very likely be new to most of our readers:

> "A hydrocele sometimes varies in size, being larger and more tense in the after-part of the day than when the patient first rises in the morning. This change has often been mentioned to me by patients, and I have lately quite satisfied myself on the point by

> getting a gentleman with hydrocele, who made an early morning visit, to call again late in the afternoon, when I noticed a marked increase in the size and tension of the tumour. The extent of surface afforded by the dilated tunica vaginalis is large, and the condition of the parts during night and day so very different that such variations in size, consequent upon alterations in the functions of secretion and absorption, do not appear at all unlikely to occur.[1] I have been informed of a case in which the change was so remarkable that the scrotum, which was full and tense when the patient retired to rest, became contracted and corrugated by the time he rose in the morning." (P. 105.)

Many readers will turn with interest to this new edition to see what judgment Mr. Curling's ample experience has enabled him to form with respect to the treatment by silver setons, which finds favour with many good surgeons. That judgment is decidedly unfavorable.

> "Dr. Young, of Edinburgh, tried metallic setons, by request of Sir James Simpson, in several cases, with success. They have also been employed by other surgeons, but not generally with a happy result, for they often give rise to active inflammation, ending in suppuration." (P. 127.)

Our own experience, as far as it goes, supports this view. Nay, we have even seen a case in which the fatal result which ultimately ensued appeared to own as its primary cause the acute suppuration set up in the tunica vaginalis by a silver seton. We are glad also to see that Mr. Curling's more extended experience only strengthens his confidence in the iodine injection as the ordinary remedy, though, as he truly observes—

> "Iodine injection is not capable of effecting a cure in every case. The judicious surgeon, therefore, whilst resorting to it as his ordinary remedy, will be prepared in special cases to avail himself of other means, such as the seton, incision or excision of the sac." (P. 139.)

A short section of this chapter contains an account of a previously undescribed affection—"inguinal hydrocele," in which a testicle retained in the groin, but having a separate tunica vaginalis, becomes the seat of hydrocele. When this very rare affection can be diagnosed (of which several instances are given) removal of the testicle is recommended.

In the section on congenital hydrocele we are happy to see that Mr. Curling has completed an accidental omission in his enumeration of the diagnostic signs by the insertion of a sentence out of the able notice of his first edition in this Review, one more instance, if any were wanted, that the masters of their art are never too proud to take a lesson wherever they can find one that is useful. With respect to the origin of the cysts in encysted hydrocele of the testicle, Mr. Curling has been led, by more extended researches since the publication of the last edition of his work, to modify his views, so as to bring them more into accordance with those of Gosselin and Luschka. The description is an interesting one, but too long for quotation. Mr. Curling seems to have succeeded, about the same time as Luschka, and in ignorance of his investigations, in demonstrating the communication between the ducts in the epididymis and the sac of an encysted hydrocele, by which the spermatozoa so often escape into these cysts. Both in this form of hydrocele and in the encysted hydrocele of the cord Mr. Curling now recommends the iodine injection, having had experience (as we have ourselves) of its efficiency. We must pass over the sections on the complications of hydrocele, and on hydrocele (true and spurious) of the hernial sac, though there is much in both that would well repay extraction and comment. In a diagnostic point of view, especially, these sections are of much interest.

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[1] These changes were distinctly noticed by Gosselin in a young Spaniard with double hydrocele.—*Note to the French translation of this work*, p. 110.

In the chapter on *hæmatocele* we notice with satisfaction that Mr. Curling dissuades the old practice of stuffing the cavity with lint, as a useless and dangerous complication of an operation not entirely free from danger under any circumstances. In very large chronic hæmatocele, in persons advanced in life, Mr. Curling considers the better practice to be the removal of the whole mass, including the testicle. In other instances he refers with apparent approval to Gosselin's proposal of "decortication," or removal of the old thickened sac. A very interesting case of hæmatocele, with inversion of the position of the testicle (on p. 216), and a very singular instance of a large hæmatocele of the spermatic cord (on p. 227), deserve notice.

Acute orchitis (chap. vi, sect. 1) Mr. Curling divides into parenchymatous orchitis, which arises from miscellaneous causes, and epididymitis, which is usually consecutive on gonorrhœa. In the severer instances of the latter disease Mr. Curling dwells on the benefit derived from tartar emetic; and he also advises, both in the parenchymatous orchitis and epididymitis, a mild mercurial course, which he believes to avert the permanent thickening of the epididymis and the risk of occlusion of the excretory duct, otherwise liable to succeed. In appropriate cases local bloodletting, ice, and compression, meet with his approval; but he reprobates the puncture of the tunica vaginalis, except in rare cases, accompanied with much pain; and is decidedly opposed to the incision of the tunica albuginea, which was recommended by Vidal de Cassis, and has lately been again brought into notice by Mr. H. Smith.

The section on *chronic orchitis* (non-syphilitic) contains a good description of this affection, which the writer is careful to distinguish (as it ought to be distinguished) from the tubercular or strumous affection with which Sir B. Brodie's method of nomenclature led many to confound it. As the two diseases are different in their pathological nature, and very different in their essential danger, it is of importance that they should not be regarded as identical. A good practical account of the treatment of "fungus," or "hernia testis," will be found here. Passing over the section on *syphilitic orchitis*, we come to a short but interesting account of *chronic infantile orchitis*, which our author wishes to class as syphilitic; and certainly all the cases which we can remember to have seen bore out this view of the origin of the disease, and were curable by anti-syphilitic remedies. The section on strumous orchitis (or tubercular in the exact sense of the term) received deserved praise from our Reviewer on the first appearance of the work, and its relative value and excellence has not diminished with the growth of the book. We then come to the malignant diseases (chap. viii). Here, unfortunately, our knowledge, both pathological and practical, has increased little, if at all, in the quarter of a century which has passed since Mr. Curling commenced his work. Little can be done in this department of surgery, as far as our present knowledge goes, beyond enumerating the signs by which incurable disease may be recognised, in order to give the patient the faint chance of immunity from return which its early removal offers. Mr. Curling gives two cases, which seem to him well-marked examples of hard cancer, or scirrhus, of the testicle. The disease, however, appears to have had few of the symptoms of cancer as it usually occurs in this organ. The ordinary (encephaloïd) cancer of the testicle is very well described here, with a view to what is, after all, the only important practical point connected with it, viz., its diagnosis. As soon as this point is established (and there is no practical surgeon who would not agree with Mr. Curling as to the difficulty of establishing it, and few who might not benefit by reading his remarks on that head), it is right to give the patient the chance of life which its removal may afford. What is this chance? Here we are glad to find Mr. Curling giving notes of four cases in which the patients remained free from a return of disease—(1) till death, fifteen years afterwards; (2) for five years; (3) for more than nine years; and (4) for twelve years; the patients being in all cases in good health at these respective periods. Authentic cases such as these would justify, and in fact oblige, the surgeon to recommend an operation; but there are other motives, which Mr. Curling does not forget to state, viz., that the diagnosis is almost always, to the last moment of the operation, uncertain, for the disease may turn out to be an innocent cystic or fibro-cystic tumour; that the operation involves hardly any danger to life; and that it sometimes protracts (and, we would add, nearly always ameliorates) the period of existence which remains. Still, Mr. Curling does not blink the undoubted fact that in most instances the disease returns, and that with great rapidity. Other forms of malignant disease hardly require more than mere mention. Melanosis seems to be merely an accidental peculiarity of a very few cases of encephaloid cancer. Colloïd cancer, which was noticed in the original work, has disappeared from this edition; and of the two cases of carcinoma of the tunica vaginalis given originally, one has been erased. The chapter (ix) on *cystic disease of the testis* is interesting, as giving a more distinct demonstration of the origin of this transformation of the organ from a dilatation of the minute ducts than was contained in the first edition, and as clearly distinguishing from each other the malignant and the innocent form of the disease, the latter of which the author believes to be the more common. We pass over the next few chapters, treating of the *fibroïd and cartilaginous tumours* of the testis, *calcareous deposits, loose bodies in the tunica vaginalis, dermoïd cysts and entozoa* in the testes and scrotum, *spermatocele, irritability and neuralgia* of the testis, to come to the important chapter (xviii) on the functional disorders, *impotency, sterility, and spermatorrhœa.* The first subject, that of impotency, is treated by Mr. Curling with a fulness which leaves nothing to desire, either in respect to the causes, the phenomena, or the treatment of this most distressing incapacity, the mere apprehension of which, and that often most groundless, is the cause of misery to so many, and has so often led to despondency and suicide. But the novel feature in Mr. Curling's work is the section on sterility in the male, a condition not ordinarily distinguished from impotency, yet extremely different, since here the power of coition is perfect, and in most cases that of ejaculation likewise, so that there is no impotency, and perhaps frequently not even any diminution in virile power; but the semen being wanting in its essential component—the spermatozoa—the connection must necessarily remain always unfruitful. The four causes to which Mr. Curling traces sterility in the male are—(1) malposition of the testicles; (2) obstruction in the excretory ducts of the testicle; (3) impediments to the escape of the seminal fluid; (4) aspermatismus, or non-ejaculation. As to the first cause, Mr. Curling (as is well known to readers of this Journal, April, 1864) believes that testicles do not secrete spermatozoa, and are consequently unfruitful whenever they do not occupy their normal position in the scrotum. At the same time he adduces and fairly states cases in which men with this deformity are said to have procreated children; only his explanation of the alleged fact is one more obvious than complimentary to the ladies whose reputation is in question. Mr. Curling himself, however, is fain to allow that there may be exceptions, and we must admit that, while there is strong evidence that many cryptorchics are sterile, we are hardly yet in a position to affirm that all are so.

On the second head, Mr. Curling gives several exceedingly interesting cases of sterile marriages, where the husband had suffered from double orchitis, and where no spermatozoa were detected on repeated examinations of the ejaculated fluid. The point is a most interesting and important one; but evidently a very difficult and very delicate examination of *all* the facts of each case would be necessary before we could tell what weight to assign to those which are here adduced. Certainly, however, the absence of spermatozoa in the repeated examinations of so many cases, is a fact to which too much weight can hardly be assigned, short, at least, of pronouncing it decisive. How do we know that spermatozoa are always to be found in every drop of the fluid obtained after intercourse by a fertile male? The other causes of sterility which Mr. Curling assigns are due mainly to chronic stricture impeding ejaculation, and to some unexplained nervous error, by which the reflex function which terminates the act of coition is not effected. Mr. Curling ends the section by stating the questions which grow out of it. They are, briefly, whether we should advise men in this situation not to marry; and whether, if married, such a condition would be ground for a divorce? The second question seems to us hardly to require to be put, so clear is the answer in the negative. A woman takes a man, as he takes her, "for better, for worse," and no "unsuspected unfruitfulness" (as Mr. Curling phrases it) which may arise after marriage on either side will ever be held a cause of legitimate separation, so long as marriage is intended to be more than a temporary concubinage. Nor does the first question seem to us precisely one for the surgeon. His duty appears plain—to explain to his patient (the expectant bridegroom) his opinion as to his probable unfruitfulness, and the grounds on which it rests, and then leave him to act according to the dictates of his own reason and conscience. For our own part, we can only say that nothing would induce us to advise a patient against marriage on the grounds alleged by Mr. Curling, although we fully allow that there is much reason for thinking that most, if not all, the persons whose state he describes would fail in procreating offspring.

The section on spermatorrhœa is in excellent taste, and written with that sound knowledge of the subject which we should expect from the author; but we must refrain from quotation. The chapter on the operation of removing the testicle terminates the part of the work which treats of the testes.

We have left ourselves but little space for observation on the rest of the work, treating of the affections of the spermatic cord and scrotum, and must be content with reference to a few of the more salient points. First, with respect to the operative treatment of varicocele. We are glad to observe that Mr. Curling's subsequent practice "has caused him to take a more favorable view of proceedings for obliterating the spermatic veins than were expressed in former writings." (P. 509.) The operation which Mr. Curling practises is identical with that recommended by Mr. H. Lee, and he justly says (p. 56) that "in healthy subjects, and with due care, the risk of pyæmia from a subcutaneous operation is extremely slight." The chapter on *retraction of the testicle* from spasm of the cremaster muscle will well repay perusal, as will the short chapter on *œdema of the scrotum*, an affection which appears occasionally to depend on constitutional causes, and to exist independently of anasarca in other parts, and which should not be confounded with the disease treated of in the succeeding chapter, viz., *diffuse inflammation of the scrotum*, the distinctive characters and treatment of which are well described. The most important topics, however, in this part of the work are elephantiasis and chimney-sweeps' cancer. The chapter on elephantiasis is good and complete, though containing less of personal experience than we should have expected from a surgeon of Mr. Curling's distinguished position and connection with a public charity so frequented by persons from hot countries as the London Hospital. We may hope that the formidable operation required in these cases will become less fatal since the introduction of the clamp or other similar contrivances for restraining hæmorrhage, although a case referred to on p. 570, in which the patient's death was caused by the instrument compressing a herniated bowel shows that the clamp is not without its compensating inconveniences. As to chimney-sweeps' cancer, little of novelty can be said about it, and we are glad to notice Mr. Curling's opinion, that the disease is decreasing in frequency since the introduction of machinery for chimney-sweeping. A few chapters on the rarer forms of tumour of the scrotum (amongst which we ought not to forget to specify *congenital vascular tumour*, a new chapter in this edition) completes a work which requires no recommendation of ours to introduce it. We cordially congratulate Mr. Curling, and hope he may long enjoy the reputation and success which this book has so fairly won for him.

Review X.

1. *Cholera in its Home, with a Sketch of the Pathology and Treatment of the Disease.* By John Macpherson, M.D., &c. —*London:* John Churchill and Sons, 1866.
2. *Army Medical Department Statistical, Sanitary, and Medical Reports.—Statistical Report on the Health of the Navy for the Year* 1862.—*Report on the Epidemic Visitation of Cholera in* 93*rd Sutherlandshire Highlanders in* 1862. By William Munro, M.D.
3. *An Essay to prove the Contagious Character of Malignant Cholera; with brief Instructions for its Prevention and Cure.* By Bernard M. Byrne, M.D., U.S. Army.—*Philadelphia,* 1855.
4. *On the Nature of Cholera, as a Guide to Treatment.* Second Issue, with a new Section on Treatment. By William Sedgwick, M.R.C.S., Surgeon to the St. Marylebone Provident Dispensary, &c. *London,* 1866. Pp. 200.

> "A lengthened experience of the disease has enabled me to observe the rise and fall of many theories and methods of treatment, and may give me some title to regard the whole subject from a practical point of view.
>
> "One main object of these pages is to show what medicine can and what it cannot effect in cholera; and their end will have been answered if they tend in any degree to check the unreasoning adoption of every possible remedy (most of them being in reality old ones resuscitated) which was displayed when cholera lately threatened us, and which, it is to be feared, will again manifest itself should cholera visit us during the course of the approaching summer or autumn."

We quote the above from the preface of Dr. Macpherson's work, which has quite recently appeared.

It does not appear that cholera was first known in the Delta of the Ganges, but that it rather settled there in congenial soil in 1817, having been described at Goa in the sixteenth and in Java in the eighteenth century.

Hot and dry months favour the severity and spread of cholera in Calcutta, as well as that of smallpox. Next in fatality from cholera are the so-called cold and dry months, whereas the least fatal are the hot and moist. Natives are more liable to cholera than Europeans in the cold months—when, we must observe, the range of temperature is greatest.

The reader will remember that, in the preceding pages, we observed that the mortality in the upper country in India differs in its seasons of cholera mortality from that of Calcutta. We there also observed, the localities differed much. In fact, it is said that in and about Calcutta the subsoil water exists within a short distance of the surface.

Now, it must be evident that in the so-called hot and dry months there must always be a vapour, which being the result of an average temperature of 86°, and rising through a soil formed of rich alluvial deposits full of organic matter, we can understand must be eminently unhealthy, and unlike what would occur in a soil of a more salubrious nature.

Fever and dysentery are least frequent during the chief cholera season, and begin to increase at the season when cholera dies away. Cholera appears to have "far more fixed rules in its home than elsewhere." Dr. Byrne, U.S. Army, observed that

> "Cholera and yellow fever prevailed together with such equal violence both in New Orleans and Tampico, that it was difficult to decide which maintained the mastery! Which of these diseases was 'the epidemic?'
>
> "Cholera shows a tendency to periodicity everywhere. Its season of prevalence in Bombay is much the same as in Calcutta. In the North-west Provinces of India, which it visits epidemically, its season is about two and a half months later than in Lower Bengal."

And, speaking of malarious influences, Macpherson says that though not reducible to laws of temperature or of electricity, of moisture or of atmospheric pressure, still their existence is generally assumed, just as that of a cholera poison; that such influences have their share in originating cholera is admitted by all Indian observers, &c. Inundations, he adds, have always been considered the great generators of malaria, when the waters begin to dry up; and that in a Bengalee village, after an inundation, there is sure to be a severe epidemic of fever or of cholera, or of both together, or of first one and then the other.

Quoting Dr. T. A. Wise, who, writing of Eastern Bengal, says, "When fever and dysentery were very common and fatal in the jungle, cholera was committing great ravages along the exposed parts of the retiring river Megna," Macpherson then goes on to ask why cholera is far more frequent in the hot months, when malaria is considered to be least so? and why has cholera become so intensified only within the last fifty years, whereas this source of the disease has always been in existence?

Now we think the rapid growth of Calcutta, which has nearly doubled in the last thirty years, being now in round numbers 400,000, as this author states, answers the latter question; whereas the explanation of the cause of the frequency and fatality of the cholera in the early hot months in Calcutta depends, as it seems to us, and as we have above said, on the effect of heat on the soil and subsoil water, and the changes effected thereby in the matters impregnating the same.

Speaking of Lower Bengal, Macpherson follows Twining and Sir R. Martin in the belief of the slight degree to which it is contagious. He has known an orderly to sleep the whole night in the bedding on which a cholera patient had just died. The medical attendants, the sweepers, and washermen, he has scarcely ever known to fall sick, although there was little or no employment of disinfectants. Now we can understand, from the power of heat in dissipating contagious matter (whatever the nature of that matter may be), that the poison may be more diluted thus, and by the almost necessity for free currents of air through a building in Calcutta at all seasons, that this matter may be so weak, that the danger of taking the disease in this way may be much less than in our own cooler climate, and in our houses so little ventilated. But we cannot sanction passing without reprehension the idea—which may easily be taken up from the work more especially under our present notice—that disinfectants, and total destruction of and scrupulous cleanliness in the removal of every particle of cholera excreta, should be used and put in practice.

We reason, as we do now, from some considerable experience of fever and of cholera in temperate as well as in hot climates. We have seen fever almost fade out in the heat, and under the effect of the more free ventilation practised in summer even among the very poorest in England, Ireland, and Scotland, to reassume formidable proportions in the succeeding winter, and more especially spring months. And we would also instance the existence of cholera on board ill-found and crowded ships, and its carriage for long distances in such; whereas, although we have occasionally met with dropping cases of it in ships of a superior class, it has been soon after leaving harbour, and always also either in persons who had committed some error of diet, as eating bad fruit, &c., and in those who had also been placed in badly ventilated parts of the vessel. Our experience, in short, goes with that of many who insist on the portability of the germ, whatever that may be, of the disease; and certainly a very large mass of evidence now exists, clearing up much of what Dr. Macpherson still regards as the mysterious progress and spread of or exemption from the attacks of cholera.

While, however, we think thus, we would be well pleased to be able to regard the prevention of the disease as a probable achievement, as advanced by a contemporary; for, greatly as we value strict precautionary measures, and assuredly as we are convinced of the power up to a certain point, we still think that, as far as our present knowledge goes, the ability to stop the carriage of disease would fail if the hitherto inscrutable atmospheric predisposition, so to speak, co-existed with the presence of infected persons. Of this, however, we greatly rejoice that experience daily gives more confidence that such a consummation of improved sanitary conditions may be arrived at, that at length what is now a hidden agency may be discovered, and general health may be so improved that the chances of any very general epidemic may be reduced to a minimum.

The returns of the Registrar-General of England show clearly that very many districts escaped during our previous experience of the disease, and this is borne out also by French experience. In the latter case the far greater liability, as with us also, of ports and towns on the great lines of traffic to the disease is seen.

A very important point mentioned by Macpherson is the great liability of "new-comers" to Calcutta to be attacked with cholera (as also those who are travelling and those who are overcrowded). Now in this respect we find an analogy between cholera and fever, as the same character is pointed out in reference to the latter by A. Buchanan,[1] C. F. Moore, and Smoller.[2]

Depression from any cause tends to cholera attack, as fatigues, excesses of every kind; sickness, as dysentery, phthisis; purgatives, if acting severely; climatic vicissitudes; exposure to malaria and fatigue combined; any irritating food, as bad shell-fish, copper cooking utensils. This is somewhat at variance with the French author who regards copper as prophylactic.

Neither race, nor strength of constitution, nor regularity of life, nor acclimatisation, nor excellence of dwelling-house, nor previous attacks, nor age nor sex, gives security against the disease, according to Dr. Macpherson. While granting this position, we would do so premising that, however, many of these circumstances diminish the probability of an attack.

Macpherson's reasoning on the theories of Snow we think somewhat fallacious, as he argues that because in the rains the excreta are more likely to be washed into the wells than in the dry weather, it cannot cause the cholera. He forgets that dilution and comparative coolness characterise the water in the rains; but in the drought the very essence of nastiness exists in the concentrated washings of the previous rains, and that the high temperature has favoured decomposition also. Nor does he regard with much more favour the theories of Budd and Pettenkofer. We must, however, admit that his objections to Snow's theory are, perhaps, more valid than might be inferred from what we have just said, as Snow rather left the idea that concentration was not required for a solution of the excreta.

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[1] 'Medical Press and Circular,' May 2nd, 1866.

[2] New Sydenham Society's 'Year Book,' 1863.

It appears to us that Macpherson has not studied what has been written on the prevalence of ague in hot and dry weather, and what, indeed, on reflection, every thinking mind must arrive at, namely, that a bad state of subsoil water will always co-exist with heat and concentration. We have not space to quote at any length Dr. Macpherson on these points. Whatever opinion we may entertain as to the identity of the old and the modern disease, it is of some importance to have ascertained that 300 years ago names still in use in India were applied to the disease, and that from the earliest date it has been treated on the same principles as those on which modern cholera has been commonly handled.

Giving the symptoms of the disease (which he considers there is evidence to prove has existed for centuries in India) as known at present, and as recorded by ancient authors, Dr. Macpherson mentions the elevation of the temperature of the body after death, as noted in Paris, in India, &c., and as observed in some cases in other diseases.

The greater severity of cholera at the commencement of an outbreak, when compared with the cases occurring towards its close, and the comparative inefficacy of treatment in the earlier periods, are constantly observed. The more slow the course of the disorder, the better is the chance of recovery.

Chapter VII contains "analogies" between cholera and the effects of hæmorrhage, antimony, Croton oil, fish-poisoning, ague, fièvre algide, eruptive fevers, extensive burns, snake-bites, external violence, and of typhoid fever, &c. In this chapter, as well as in the preceding one, the author quotes Hippocrates, Riverius, Sydenham, Willis, D'Orta, Bontius, Celsus, Buhl, Gull, Sangbusch, Beck, Marshall Hall, Burns, C. W. Bell, Haspel, Martin, Elliott, Perkins, Kennedy, Orton, &c., and concludes by saying—

> "The secondary stage of cholera in some of its forms, with its exanthema, does really come very near typhoid.
>
> "On reviewing the analogies that have been thus presented to us, one can scarcely resist the conclusion that the general analogies of cholera are much more with acute poisonings and hæmorrhages, and with the malarious rather than with the eruptive type of fevers.

> "It might be possible to mistake the collapse of fièvre algide for that of cholera, and the secondary form of cholera for typhus; but it would be impossible to confound any stage of the disease with one of the eruptive fevers: yet the generally assumed similarity in the nature of cholera and that class of blood-diseases must not be wholly overlooked."

The same author points out, as we ourselves have above indicated, that much of the variance of opinion on the pathology of cholera depends on whether it is viewed in its more sudden or in its more gradual form.

Commencing with the Hindoo, Greek, and Arabic schools, in whose wake followed Riverius and Sydenham, and who regarded the disease as a poison and its symptoms as the efforts of the system to get rid of the same, Dr. Macpherson gives a succinct and excellent analysis of the several theories that have been from time to time proposed of its pathology. Jameson, in 1820, regarded the stomach and intestines as the primary seat of the disease: he did not believe in suspended action of the circulation or spasm of the extreme vessels, which "are rather in a state of atony than of spasm."

In 1819 the Bombay Medical Board said that—

> "Cholera resembled the effects of a poison taken into the stomach or applied to the blood; but whether it acted more immediately on the circulatory system or on the nerves they could not determine."

The old idea of a blood-poison was early revived in India, and the vomiting and purging were looked on merely as an effort of nature to eliminate the poison. Kennedy, in 1827, reasoned thus, regarding the nervous system or great sympathetic, however, as the first seat of the shock. Subsequently Magendie attributed the disease to weakened power of heart, Ockel to circulatory paralysis, Delpech to the great sympathetic, Foy to the spinal column, Broussais and his followers to inflammation of the intestinal canal. "Perhaps Rochoux (writes Macpherson) took the most philosophical view of the case," as he thought it an alteration of the blood by a deleterious agent acting specially on the nerves of circulation and respiration, and on the mucous membrane of the intestines.

Passing in review the theories and writings of D'Orta, Davy, Rayer, Twining, Boehm, Mackintosh, Gluge, C. W. Bell, Parkes, Buhl, Gull, G. Johnson, and Griesinger, Dr. Macpherson observes

> "We are, after all, brought back very much to the views of Rochoux and the Bombay Medical Board. We do not doubt the existence of a blood-poison; but as we do not know its nature (very possibly it may be a ferment, some nitrogenous organised body), so we cannot

yet talk with precision of its *modus operandi;* and the assumption that there is a poison present does not necessarily tie us down to accepting only one method of its operation, nor to regard vomiting and purging necessarily as efforts of nature to eliminate a poison."

The evacuant treatment, which is of very old date, was first opposed by T. Willis; and from his time two opposite views of treatment come constantly before us, one aiming at the diminution of exudation, the other being eliminative,—just as ague formerly and typhoid latterly have been treated (Dr. Macpherson observes) on like modes of practice. He does not attribute secondary fever to the action of medicines, and gives several interesting cases in which large and repeated doses of powerful remedies, as calomel, opium, &c., produced no apparent effect at all.

Reasoning on the indications of treatment in other diseases, he shows the fallacy of considering it wrong to cut short a disease or to blindly pursue a so-called eliminative treatment: he states that the action of quinine has no satisfactory explanation.

A list of the many remedies that have been tried is given under headings denoting the intention with which they were given. The author goes on to argue, as we have done above without being aware of his statement, that we do not seek to eliminate typhoid poison by an active drain, or smallpox by frictions with croton oil, as has been mistakenly attempted, &c. Dr. Laycock writes of the early stage of cholera, "Charge your patient, as he values his life, not to irritate his gastro-intestinal mucous membrane." Other authors are quoted to the same purpose. Gubler also does not regard the combination of emetics and purgatives, as used in India and in Paris lately, as satisfactory. Castor oil, so much condemned by the great majority of the profession as dangerous, and as having produced abundant diarrhœa followed by cholera in a case cited by Valleix from his own practice, Dr. Macpherson can see no reason for using.

Twining, Bankier, and others, as well as Macpherson, condemn even gentle purgatives in cholera, and in the recovery stages of the disease. We saw a fatal case of choleraic purging with collapse, &c., in the autumn of 1865, although there was no epidemic of cholera in the locality, apparently induced by the use of aloetic pills. In short, as the author of 'Cholera in its Home' observes, the misapplication of purgatives is often irremediable.

Emetics are not in favour; but to husband the strength of the patient, and that by opium, Macpherson regards as the best line of treatment, quoting Twining, Orton and Jameson, Gubler, &c. This drug he considers with the latter author as assisting the action of stimulants, astringents, carminatives, or absorbents.

The great point is to treat the early or diarrhœa stage, which Macpherson regards as premonitory of the cholera.

In the "invasion of the disease" he still uses opium in the form of laudanum, small repeated doses of chloroform, and even inhalation of chloroform; also chalk powder, magnesia, or bismuth, as well as sugar of lead and vegetable astringents. Sometimes he has thought injections of cold water into the rectum, or of solutions of sugar of lead or nitrate of silver, useful. Stimulants he also recommends in this stage, both internally (as wine punch, ethers, ammonia, &c.), and by external frictions of cajeput oil, dry ginger powder, turpentine and oil, &c.

Long before the collapse stage he gives up opiates; he considers it uselcss to give drugs in this stage, "in hopes of obtaining from them their specific action;" but drinks in small quantities to relieve thirst, and stimulants in small quantities, are to be used, watching their effect on the pulse and on respiration.

In common with many others, he advises small quantities of ammonia. He discountenances violent measures, as blistering the spine, applying the actual cautery, &c. In the sequelæ of the disease, as in the stage of recovery, meddlesome medicine is injurious.

In conclusion, Macpherson rightly directs attention to India as the home of the disease, insisting on sanitary measures there, and in the ships used by pilgrims

The 93rd Highlanders suffered from cholera in 1862, when stationed in Peshawur, in the Punjaub. It is situated in an extensive valley, and there is a large marsh "close to cantonments." Irrigation, by means of dirty open ditches, the latter many miles in extent, is carried on through cantonments, gardens, streets of the city, &c., the ditches being on a plain higher than the foundations of the houses; and one of the largest and dirtiest of the ditches runs close past the barracks and hospital which were occupied by the 21st Hussars, and also close past the barracks occupied by the 93rd, its bed being higher than the foundation of the barracks. In winter the season is wet, cold, and attended with cutting winds from the mountains; in summer there is great heat by day, followed by cold, damp nights. Dust-storms are not common, nor are thunder-storms.

Cholera first appeared in a native regiment stationed at Kolrat, but ceased in the cantonments of that place on the affected corps being marched out and encamped about three miles out. Other corps were subsequently attacked, but the disease ceased on their being marched out several miles and encamped.

About the same date cholera appeared in the city of Peshawur. Dr. Munro speaks of the choleraic influence or atmosphere moving in a north-easterly direction, &c.

Every precaution was taken to prevent, as far as possible, the troops suffering from the disease: strict cleanliness, flannel belts, recreation, avoidance of exposure to sun and to night air as much as possible, &c., were among the measures adopted. In all, 443 men were sent in detachments to Chorat, 3000 feet above the valley. These measures left the head-quarters of the corps much more room.

There were four distinct outbreaks of cholera in the regiment. The first was from 7th July to 17th ditto; there were in it fifteen cases of cholera and twenty-two of choleraic diarrhœa. None of the latter ended in cholera. The second began 26th July and ended 10th August. Twelve cases of cholera and twenty-nine of choleraic diarrhœa. Of the latter two ended in cholera. The third lasted from 9th September to 16th ditto, and embraced nine cases of cholera and two of choleraic diarrhœa. Of the latter none ended in cholera. The fourth began on 12th October and ended on 3rd November, when the regiment left the valley of Peshawur. On this last occasion there were fifty-seven cases of cholera at head-quarters and six absent.

The regiment was moved out into tents on the parade-ground on the second outbreak of cholera. During the first and second outbreaks there was a cessation of fever; but during and after the third and fourth outbreaks there was an increase of remittent fever; and after the disappearance of the cholera intermittent fever became the prevailing disease. Close to where the first case occurred were two large water-courses; and the second building in which cases occurred was close to the hospital and to a large dirty stream of water.

The greatest number of cases occurred in men twenty-five years old, and in those longest in India. As long as the officers remained in "their well wooded compounds" (and comfortable bungalows) they escaped entirely; but, three days after going into camp in October, five officers were attacked, and four died, &c. We have long had great doubts of the propriety of marching out troops, regardless of weather, in India or elsewhere, and we think this specimen of its effects decidedly affirmative of our views. The women and children suffered much. Men were almost invariably taken ill at night, or very early in the morning. To exposure to night air, when weak and fatigued, in consequence of marching out of Peshawur at 5 p.m., Dr. Munro very justly attributed much of the cholera, diarrhœa, and fever. On leaving the Peshawur valley the cholera ceased; and on crossing the Indus the whole regiment felt their spirits and health begin to return, and the feelings of nausea, cramps, and griping pains left them.

Dr. Munro states that diarrhœa generally preceded cholera, the former generally preceded vomiting, sometimes for days; vomiting was not always present. Collapse may be the first symptom without antecedent vomiting or purging: these cases were generally rapidly fatal, and attended with blueness before death. Vomiting and purging may follow collapse. Cases without premonitory diarrhœa, but attended with free vomiting, either did not become collapsed at all, or the collapse was short, and followed by a short reactionary stage and rapid convalescence. These cases were generally favorable, even though attended by reactionary fever.

On the contrary, cases with premonitory diarrhœa for several days, followed by vomiting, had all the different stages in a severe and prolonged form, and mostly died of cerebral congestion. The shorter the collapse stage the more favorable the prognosis.

"In cholera urine is often secreted without our being aware of it." Dr. Munro adds, the necessity hence of often passing the catheter, as recommended by some other writers also. Bleeding was tried in three cases, but all proved fatal. Hot baths also proved useless in collapse.

The disease did not spread to the hospital attendants. The treatment was in all diarrhœa cases mustard emetics and a stimulating draught, with brandy and water, Tr. Opii, and Ext. Zinzib.: the patient was put to bed, and had sinapisms to the abdomen, followed by turpentine fomentations. Generally this was all that was required: if he got worse, the external use of mustard and turpentine was continued, and Ether, Tr. Opii, and ginger and camphor, were given in a draught every half hour. If collapse came on, the whole trunk and lower extremities were covered over with mustard and capsicum, blankets, hot bottles, bags of hot sand, &c., were applied. Enemata of beef-tea, and turpentine and Tr. Opii, were given per anó with the long tube, and stimulating draughts continued, and thin arrow-root and brandy and soda water given ad libitum every five or ten minutes. The douche gave doubtful results in collapse. When reaction set in the head was shaved, and a wet cloth applied, and camphor, calomel, and henbane, given in pill every hour or two, and draughts of ammonia, nitrous ether, and camphor and nitre. If headache and congested eyes occurred, leeches to the temples, douche to the head; and, if still necessary, a blister to the scalp, and ointment of tartar emetic, or sabine, or blue ointment, was applied, till free discharge occurred. Beef-tea, chicken broth, arrow-root, and wine, were given.

Chloroform was inhaled sometimes with marked benefit in severe cramps (we have known[1] it applied on lint also in India with benefit in cramps). Solid opium often succeeded in hiccough in Dr. Munro's hands. Any solid food was dangerous in early convalescence.

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[1] Dr. Thomas Bray, Bnegal Medical Service, speaks highly of chloroform applied in this manner.

When mercury acted cases recovered; in fatal cases it had no effect. Of 130 men, women, and children, 89 died and 41 recovered. Of 137 cases of choleraic diarrhœa, 4 died.

Dr. Byrne brings forward evidence to prove the contagiousness of cholera; and taking up a position opposed to many other writers, he advocates strict quarantine. He also speaks highly of bleeding robust persons in the earlier stages of cholera, and condemns an opposite treatment; he condemns bloodletting, however, in collapse, stating it to be fatal. "Our principal aim should" (then) "be to raise the sinking powers of the system by stimulants, while we endeavour, at the same time, to equalise them, by the exhibition of calomel." We need hardly observe that much of his treatment would not meet with approbation in Europe.

Ice to the spine, creosote, and phenic acid in solution, with thirty or forty parts of water and some syrup, are among the remedies now proposed in cholera. Creosote was used years ago. One writer (we are, however, happy to say not a professional one) actually proposes to drive air into the œsophagus and stomach of the unfortunate patient, as if the heart's action was not weak and impeded enough already in cholera without such proceedings.

Though we regard early treatment of vital importance in diarrhœa in times of epidemic cholera, we cannot accept Dr. Macloughlin's views that it is always present as premonitory of cholera.

Acknowledging the undoubted benefit conferred on humanity by the "Snow theory" of cholera, we must regret, with the 'Medical Times and Gazette,' that the able author of it had not lived to more fully develope it, and to modify his tenets on the cell-theory.

We read with deep interest the valuable communication of Dr. Munro, as given in the army statistical, sanitary, and medical reports; and we regard them as pregnant with important matter on the causation, prophylaxis, and treatment of cholera. We see in them strong evidence of the care required in moving men into tents, in the hour of marching, and of undergoing much exertion, when in feeble health and exposed to a variable climate; and we cannot avoid expressing strongly our conviction that those weighty and all-powerful (for evil as well as for good) things, general orders, should be subject, from time to time, to reconsideration by those in authority on deliberation with medical men.

On the subject of treatment in cholera over-stimulation, either conducted by internal means or by external applications, may be overdone, especially by means such as hot blankets, or blankets in conjunction with bags of sand, as these act too like hot and vapour baths, and so oppress the system beyond its powers, as does also the too much fussiness—*nimia diligentia*, perhaps, would be a more professional term—of well-intending but injudicious persons. When saying this we would by no means forbid, but rather encourage every legitimate and sustained effort being exerted to save life, which may often be accomplished, even in apparently hopeless cases; but, as we have before intimated, our zeal must not outrun our judgment.

We must remember also that persons in cholera are very much exhausted, and that the lungs, being much hindered in their efforts to purify the blood, the greater burden is thrown on the skin, and, consequently, we should neither smother up the patient by a crowd of heated blankets, by impure air, as in a crowded room, nor should we in the same way impede too much the skin's action, and we should lessen the irregular distribution of heat in the system by unirritating wines, warm or cold, or diffusible stimulants, and by ice, cold pure water, &c., as well as by occasional cleansing the pores of the skin, light rubbing with either dry warm clothes or warm sponging, &c. We think, in short, that two great points are to be kept in view, viz., the impeded respiration and circulation, and the poison or injury done to the blood and nervous system.

We would direct attention to a report of the good effects of strychnia in cholera, as used by Mr. Hensman, of the 20th Regiment, at Yokohama; he would graduate the dose to the nature of the case in severe collapse, using a quarter of a grain every half hour. We think with the annotator of the report, and with Mr. Hensman himself, that the dose he gives is large. Thus briefly drawing attention to Mr. Hensman's communication in the 'Army Report' for 1863, we will only add that strochnia is not an agent to be lightly had recourse to being so deadly a poison, and we must remark that since writing the above we have noticed that it was lately tried without any good result in some cases that subsequently proved fatal.

We had intended to have noticed Mr. Sedgwick's interesting volume along with others which have recently come to hand, but have been obliged to postpone their consideration until our next number. We would, however, advise the reader to make himself acquainted with it forthwith, as he will find it to contain much that is most useful and suggestive. The first edition was favorably noticed by us in a former number.

PART SECOND.

Bibliographical Record.

Art. I.—*On Disease of the Right Side of the Heart.* By T. M. Daldy, M.D., late President of the Hunterian Society. Pp. 71. *London:* 1866.

This unpretending little work aims at calling the attention of medical pathologists to a cardiac lesion of which we can learn scarce anything from the pages of former writers on the heart, namely, imperfect tonicity of the muscular fibres of the *right* auricle. The other points out the impediments to circulation which must arise from the presence of such an imperfection, and he thinks it is oftener present as an original lesion than has been usually thought. We are all, of course, familiar with dilatation of the auricle as a consequence of obstruction to the further course of the blood either through the left heart or through the lungs, or by reason of alterations of the tricuspid valve. What Dr. Daldy would call the attention of the profession to is something very different from this, namely, an idiopathic feebleness of the auricle without any permanent lesion in the rest of the organ. Of this condition he does not lead us to expect ocular demonstration in an enlarged cavity of the auricle as exposed at a post-mortem examination. He thinks the morbid state is not "dilatation," but "dilatability," and that the enlargement of the area may exist *during life* only. Not but what he looks to a possibility of the state of the muscle presenting even after death evidence of imperfection, and he calls upon microscopists to inquire into this part of the subject.

If it may be allowed to critics to whom these views are new to make a suggestion to one who has been thinking about them a great many years, we would take leave to propose his injecting the heart with tallow at a definite strong pressure. The comparison of a healthy heart so treated with that which he suspects of dilatability would afford the necroscopic test Dr. Daldy wants.

The pathognomonic signs of the condition referred to he considers to be "dulness on percussion to the right of the junction of the xiphoïd cartilage with the lower third of the sternum, and the propagation of the heart sounds in the direction of the right clavicle, conjoined with the derangement of the circulation of one or more distant organs." He gives an example of a physician, aged 27, who tried for a joke to carry a boy of 16 up an embankment near the Crystal Palace, and who was nearly moribund afterwards. Dr. Daldy the next morning found the above-named evidences of largely-distended right heart. "A few days of absolute quiet restored him, and he is now quite well." We quote this case to show again that the state suggested by Dr. Daldy as being frequent is a temporary, not a permanent, one.

The difficulty we have in our experience found in the diagnosis of dilated right heart is that strong expansion of the lung, such, for example, as takes place in asthma, displaces the organ downwards and to the right side. Even a violent voluntary inspiration in a healthy thin person will cause the beat to be felt in the epigastrium. Moreover, general enlargement of the heart throws it across the sternum. Besides which the stomach after a full meal, at which period these attacks usually occur, assists in the displacement. We think the author overrates the facility of diagnosis.

Dr. Daldy believes this disease to be hereditary. He gives an instance of his having in a woman, anæmic from post-partum hæmorrhage, diagnosed *hereditary* feebleness of the right auricle, and his receiving the prompt statement that her parents were quite healthy. Yet he adhered to his opinion, and luckily found downstairs the patient's mother, who confessed "Oh, yes, I have suffered a great deal here," pointing to her lower sternal region "for years; and when I was pregnant with her, my heart was a constant trouble to me; not from palpitation, but from a feeling of sinking and short breath. My father was so before me."

The results which he traces to debility of the right heart are (a) habitual congestive headache; (β) vertigo; (γ) softening of the brain; (δ) mania. Of a he gives an example in which he advised sleeping with the feet in a dependent position, but the suggestion was not carried out. Of β there are two cases where he has detected an habitually distended auricle. With γ and δ the connection of the lesion is rather inferential than experimental. Dr. Daldy once took a summer residence near a lunatic asylum for the purpose of investigating the matter, but was foiled by the resistance of the inmates to stethoscopic inquiry.

As to treatment, Dr. Daldy recommends in the first place "mens sana," an absence of "res angusta," and a life of ease. To "innocently diminish the quantity of blood sent to the right side of the heart," he would give Püllna and Vichy water; to impart muscular vigour, iron; and where the nerves suffer, quinine and nux vomica.

Altogether, the volume is suggestive rather than dogmatic.

Art. II.—*Some Remarks on the Nature and Treatment of Pulsating Thyroïd Gland, with Exophthalmos ("Graves' Disease").* By William Moore, M.D., &c. *Dublin.* Pp. 12.

This paper of Dr. Moore's is a valuable contribution to the pathology of a very singular and obscure disease, and will well repay an attentive perusal.

Six cases of it have come under his observation, five females, one male; one only of them terminated fatally.

After describing the probable causes of the malady, detailing its symptoms, and adverting to the opinions of those writers who have discussed its nature—these differing materially—he offers his own; which is, that the affection is essentially nervous, the vaso-motor system playing the most conspicuous part as to its origin; and that the cardiac pulsations, pulsating thyroïd gland and carotids, with exophthalmos, dilated and restless pupils, are due to the increased action of that part of the nervous system above mentioned. This view, we may remark, seems tolerably to accord with the presumed causes and the symptoms generally, and also with the organic changes which accompany their progress, and the results of treatment.

Dr. Moore considers the disease far from intractable, but very apt to relapse, and to eventuate, sooner or later, in organic lesions of the parts most implicated. The medicines which he has found most useful, and on which he relies most, are tincture of digitalis and bromide of potassium; the tincture in doses of from fifteen to twenty-five drops, three or four times a day; the bromide, in from fifteen grains to half a drachm, three or four times daily; these, combined with iron, where there is suspicion of the existence of spanemia. The bromide, he recommends, as exerting a sedative and equalising effect on the vaso-motor system generally, more especially in restoring the uterine functions to their normal condition—but how, he very wisely does not attempt to explain.

Art. III.—*A Course of Practical Chemistry arranged for the use of Medical Students.* By W. Odling, M.B., F.R.S. 2nd Edition, pp. 241. *London:* Longmans. 1865.

In noting a second edition of Dr. Odling's most useful 'Course of Practical Chemistry,' it is not necessary to do more than point out the improvements which the author has effected in the work.

The first chapter is new. One of the sections into which it is divided contains a highly condensed, yet clear, account of the meaning of the chemical terms—" atom," " equivalent," " symbol," " formula," " reaction," &c.; the other section is devoted to the explanation of the structure and use of the various pieces of apparatus employed in the course of analysis. Of this apparatus woodcuts are given; these engravings are generally intelligible, but they present no artistic excellences, unlike the clear and beautiful drawings with which French works on chemistry are usually adorned.

The second chapter of the book, relating to the course of qualitative analysis, has been rewritten and greatly improved. The processes of separation and identification of the different basic and acid constituents of salts have been rendered at once simpler and more exact. While analytical tables are given, the student is not left to go through the operations indicated without a word of explanation, but for each step a reason is assigned. This we consider to be a marked improvement upon the more usual plan.

The third and fourth chapters relate to toxicological and animal chemistry. Considering the scope of the work and the necessary limitation to its size, the particulars given concerning the detection of poisons in solid compounds, in solution, and in organic mixtures, are adequate for the purpose in view, and are arranged with care and skill. Not the least important feature of this part of the volume is the paragraph describing the impediments to satisfactory testing in each case. The woodcuts which illustrate this part of the work are more satisfactory than those occurring in the second chapter. Figures 53 (nitrate of urea) and 66 (mixed mulberry and fusible calculus) are good examples of these illustrations.

Save in a few tables of atomic weights and of important formulæ in an Appendix, this work is written in the older notation. But the nomenclature is that now adopted by the advocates of the unitary notation, while no change in the formulæ is generally requisite save the halving of the numbers below the symbols of the majority of the elements, thus—$H_2S_2O_8$ becomes

H_2SO_4, and is then correct, according to the new views. Till the examining boards recognise the unitary system, students must be content with the older and empirical method.

ART. IV.—*On the Use of Carbolic Acid.* From the 'Proceedings of the Odontological Society,' June 5th, 1865. By Mr. WOODHOUSE and Mr. GIBBONS.

FROM the statements made in this paper by the authors, illustrated by cases, it would appear that carbolic acid or phenole, is very useful in dental surgery, applied with caution, by means of cotton-wool, care being taken to avoid touching the lips, which it excoriates. It is described as relieving pain without occasioning inflammation; and that where suppuration has set in, it arrests that process. It is stated to be pre-eminently useful in cases of exposure of the pulp. For minute details and precautions as to the mode of employing it, we must refer to the paper itself.

ART. V.—*Report on the Causes of Death among the Assured in the Scottish Equitable Life Assurance Society from* 1831 *to* 1864, *being a period of thirty-three Years. Submitted to the Board of Directors.* By WILLIAM ROBERTSON, M.D.—*Edinburgh,* 1865. Pp. 47.

THIS well written report is deserving of the attention of actuaries and referees of Life Assurance companies. In it Dr. Robertson shows what little reliance is to be placed on the nomenclature of fatal diseases in relation to their causes as officially returned, *post-mortem* examinations being comparatively rarely made, and in so many instances the name given by the medical practitioner being oftener a matter of conjecture than of exact knowledge. This is strikingly exemplified in the tabular views of mortality inserted. Thus, in those of England and of five Scottish assurance offices for a period of seven years, the per centage of fatal disease of uncertain seat is for England as high as 19·776, and in those of the offices from 14·3515, the highest, to 5·9506, the lowest. Nor, indeed, so long as "old age," "atrophy," "debility," "scrofula," "syphilis," "malformations," "sudden deaths," are allowed to be used by returning officers in their certificates of deaths, is this class of diseases of uncertain seat likely to be less numerous.

The tables given of mortality-experience are deserving of the notice of the statistician. Our limits do not permit of our commenting on them, or on the brief remarks of the author on the diseases to which the mortality has been owing; however, in justice to him, we can honestly say that they are always judicious, and often valuable as controverting doctrines either of an unsound kind, or not established by fact; such as the opinion that consumption, fever, and the like, on one hand, and malignant diseases on the other hand, are equivalents.

ART. VI.—*The Personal Responsibility of the Insane.* By JAMES F. DUNCAN, M.D., &c., Dublin. 1865. Pp. 98.

ON opening this little work our instant impression was that its title—'The Personal Responsibility of the Insane'—is a misnomer, inasmuch as the term itself implies irresponsibility. Our next impression was one of regret arising from the author's statement in the preface, that the volume he puts forth is "intended rather as a popular essay than a regular scientific treatise," believing as we do, from the nature of the subject, that it is ill adapted for any treatment but the most scientific; and, after the perusal of its pages with due attention, we must confess that we have not found reason to alter our opinion. Having said thus much, we further regret to say that we consider the publication altogether a mistake, and, as we think, more likely from the manner in which the subject is discussed, to perplex and bewilder than to enlighten unprofessional readers into whose hands it may happen to come. Whilst it is well written in flowing words and well-formed sentences, it is neither distinguished for originality of observation on logical reasoning, nor for the absence of truisms and solecisms.

The author dedicates his book to the Lord Chancellor of Ireland, and ends it with a summary of his conclusions, eight in number, which he offers with a degree of confidence that rather surprises us, considering the difficulty of the inquiry and the caution that should be used in conducting it.

ART. VII.—*A Treatise on the Chronic Inflammation and Displacements of the Unimpregnated Uterus.* By WM. H. BYFORD, M.D. *Philadelphia,* 1864.

2. *The Practice of Medicine and Surgery applied to the Diseases and Accidents incidental to Women.* By W. H. BYFORD. *Philadelphia,* 1865.

THE author of the above works is professor of obstetrics and diseases of women and children in the Chicago Medical College, and is evidently possessed of much practical knowledge in the department of his profession he has chosen, although, as a writer, the brevity so necessary for distinctness of meaning and clearness of expression cannot be attributed to him. The first book mentioned is so completely imbodied in the second that it is only necessary for us to notice the latter; and the subjects embraced in this are so well understood, and have been so thoroughly described by English authors, that no very lengthened remarks would benefit the readers of our review. The object of the author has been to furnish to the student and junior members of the profession a concise, yet sufficiently complete, practical, and reliable treatise, to meet their wants in everyday practice, and his object has been very fairly accomplished. The work treats, in succession, of diseases and accidents of the labia and perinæum, diseases of the vulva, vaginitis, disorders of menstruation, uterine inflammations and displacements, cancer, tumours, puerperal fever, &c., and each subject is dealt with as if by a man of experience.

The more elaborate portions of the work are those which relate to chronic inflammation of the uterus and cervix, and to puerperal fever. The effects of chronic inflammation of the uterus and cervix upon other organs, and upon the system generally, appear to us to be somewhat exaggerated, although we admit there is much truth in the author's statement that "these diseases are often overlooked, misunderstood, and neglected, and that an immense amount of suffering that is now borne as a necessity by women might be relieved, if we would investigate and study their ailments with as much patience as, and with no more reserve than, we approach and investigate lung diseases or throat affections." The sympathetic disorders chiefly referred to as consequences of chronic inflammation of the uterus and cervix are functional disturbance of the stomach, bowels, and liver, cephalalgia, affections of the spinal cord, pains in the pelvic region, extension of inflammation to the bladder and rectum, hyperæsthesia, anæsthesia, spasms, and manifestations of moral and intellectual perverseness. Many other ill consequences are dwelt upon, probably with more importance than they deserve; the hobby is ridden too hard, but the subject is well deserving of consideration, and the remarks of the author are worthy of attentive perusal. With respect to puerperal fever, the following remarks are judicious, and should oftener be acted upon than is usually the case:

> "A very important part of the treatment of puerperal peritonitis is prophylactic or preventive; and as epidemic influence constitutes the principal and efficient cause in many instances, it should be our

object, firstly, to avoid it; and secondly, when we cannot do this, as nearly as possible to counteract it. Probably the only way to fulfil the requisitions of the first proposition is to remove our patient entirely beyond the infected district. We can more effectually and certainly accomplish this with pregnant women than patients predisposed to attacks of any other form of epidemic disease, for we know precisely when to expect an attack of puerperal peritonitis, and, fortunately, we can know this for weeks, nay, even months beforehand. The woman will not be attacked until she is delivered. When any other epidemic prevails in a community everybody is liable to it at all times; every sacrifice ought, therefore, to be made to send our patient, several weeks before the expected confinement, entirely beyond the region endangered, and allow her to remain there until the circumstances which render her susceptible have passed away."

These observations are followed by others of much value in protecting patients from the action of epidemic influence; but few of our readers will admit, at the present day, that the *curative* treatment advised by the author is either safe or practicable. His principles of treatment are, first, that it must be instituted as soon as possible after the attack—when practicable, contemporaneously with it; secondly, that an *immediate* and *powerful impression* must be produced by it. The first proposition no one will dispute, but, on the contrary, most will agree with the author that whatever is effected in the way of a cure must be done generally in the first twelve hours after the attack; but when he goes on to say there are but very few remedies sufficiently prompt and powerful in their action to accomplish favorable results, probably but one—venesection—and that we may regard bleeding and emetics as the only remedies capable of interrupting the disease, we believe that, in this country at least, there will be but few practitioners sufficiently bold to bleed a patient with puerperal disease, "in the sitting posture, with a stream as large as the vein will allow, and until there is an approach to syncope, and as soon as reaction occurs repeat it." Our experience of puerperal diseases, as they occur in this country, would lead to a very different mode of treatment—an anodyne and supporting system; bleeding, except on certain occasions locally, being in almost every instance inadmissible.

Art. VIII.—*Elements of Physics, or Natural Philosophy. Written for general use, in non-technical language.* By Neil Arnott, M.D., F.R.S., &c. Tenth and completed edition, Part 2. *London,* 1865, pp. 325.

This second part or volume, is deserving of the same commendation as that which we bestowed on the first, being, from its style, aptness of illustration, and clearness of explanation, equally well fitted to introduce the student to the marvellous subjects of which it principally treats,—heat, light, electricity, magnetism.

In the opening section the author defines his intent, viz. not to deal with the question of causation, yet unsettled, relative to these imponderables, as they are sometimes designated, which can interest only a small class of readers, but to give such knowledge of important facts and laws as befits the general student; an aim, which we think he has very happily and successfully attained. Yet, in justice to him, we should add, that whilst he avoids entering into discussions relative to the nature of heat and its correlatives, the great agents already mentioned, light, electricity, magnetism, he makes known the several views entertained by philosophers respecting them.

It is no small recommendation, in a work of this kind, that whilst it instructs on matters the most abstruse, it excites an interest little inferior to that which is felt in the perusal of a story of romance; and further, that whilst it offers a sketch of the knowledge constituting the physical sciences, acquired by laborious research, it shows how that knowledge has been gradually attained, and the progress, with its acquisition, of man from a rude, primitive state of society, to his present advanced and enlightened stage.

That a work so comprehensive should be free from error, is too much to expect; but, so far as we have consulted it, especially those portions of a physiological bearing, they are few and for most part comparatively trivial; thus, as regards animal heat, he inculcates the old doctrine, that it is owing to the union of oxygen with carbon in the lungs, and that the temperature of man is no higher in a tropical than in a temperate climate.

The following extract is given as an example of the style and popular manner in which the work is written. After showing how mother-of-pearl owes its vivid colours and beauty to its minutely furrowed or striated surface, which may be imparted to wax with like colouring effect, he proceeds:

"In the mean time the investigations in progress respecting the phenomena of light are furnishing new proofs of the marvellous simplicity of nature amidst its boundless extent and most curious variety. When men thought of the sense of touch, chiefly as produced by pressure on the tips of the fingers, or elsewhere on the skin, they were far from suspecting that the sense of hearing had the near relation to it which subsequent discoveries have proved. namely, that it is only a more soft and delicate pressure, made by undulations of the air or other substances on nerves protected within the cavity of the ear; and still less did they suspect that the sense of sight was but yet a finer touch than hearing, produced by still more subtle vibrations of a medium of light on the interior nerves of the eye. But step by step they have ascertained the facts mentioned; and it is a curious resemblance that, while in sound, different tones or notes depend on the *number* of the vibrations in a given time, so in light do different colours seem to depend on the *number* and *extent* of the vibrations of the more subtle medium on which the phenomena of light depend. The human imagination cannot picture to itself a simplicity more fruitful of marvellous beauty and utility than all this. And yet farther, as air answers in the universe innumerable important purposes besides the carrying of sounds, so also does the medium of light minister in numerous ways, as in connection with the phenomena of heat, electricity, magnetism, and life."

ART. IX.—*A Proposal for the institution of Degrees or Certificates of Qualification in State Medicine at the Universities of the United Kingdom.* By H. W. RUMSEY, F.R.C.S., Member of the General Medical Council, &c. Pamphlet, pp. 9.

RESPECTING the propriety of the proposal brought forward in this ably written pamphlet, we think there can hardly be a doubt. As civilisation advances, and the social state of the country becomes more complicated, new wants arising and new evils, means are needed to supply the one and correct the other. There is, accordingly, as our author states—

> "A demand growing year by year, not simply for officers of health, medical coroners, medico-legal experts, public analysts, statistical inquirers, medical inspectors, visitors and certifiers, under various protective and preventive enactments, but rather for a trained body of scientific men fully competent to act in some or all of these capacities—men possessing higher and more special and better qualifications than have been hitherto demanded of persons holding such offices or executing such duties."

This granted, the necessity of a special training to qualify for the efficient performance of the several functions falling to the lot of the officials just adverted to seems manifest, and as manifest that it should be different in many respects from that of the ordinary medical practitioner, whether surgeon or physician; and when attained and tested by a careful examination, should be certified by a degree or degrees somewhat after the manner already adopted in the University of London, substituting for Bachelor and Doctor of Science that of Bachelor or Doctor of Civil State Medicine.

Some of the qualifications are thus sketched by the author:

> "Every officer of health ought to be fully competent to investigate the predisposing and determining causes of disease, whether hereditary or constitutional, zymotic or local, alimentary or industrial. He should also be familiar with the right use of the numerical method (vital and sanitary statistics), in estimating the effects of those causes upon the lives and health of the community. The forensic medical officer should possess such a knowledge of psychology, in its relations with biology and pathology, as would make him, obviously to all, the proper authority to report or certify officially as to the mental or physical condition of those persons whose capability of self-control or whose fitness for duty of any kind may be questioned. He should also be skilled in the laws of evidence. Further, there might reasonably be required of every one who professes to determine the cause of death, when it becomes the subject of legal inquiry, a far more extensive and minute study of medical jurisprudence and toxicology than is necessary for the ordinary physician."

The state medicine in question, as propounded by the author, might, as he suggests, form a special department of instruction in connection with the natural sciences and medicine in our universities, and in those of Oxford and Cambridge might enter into the curriculum for honours.

Whilst fully sensible of the need which exists of the class of officials such as Mr. Rumsey desires to see established, we cannot but entertain one doubt, and this as regards remuneration. As the acquirements of the officials desiderated should be of the highest order, and attainable only by men of more than ordinary abilities, and at a more than ordinary educational cost, we very much fear that the majority of men would prefer the profession which holds out the best prospects of emolument, especially in this country, in which wealth is so much considered, in which science is so coldly regarded by the Government, is so little honoured by the public, and in which funds for scientific research are so grudgingly bestowed.

ART. X.—*Contributions to assist the Study of Ovarian Physiology and Pathology.* By CHARLES G. RITCHIE, M.D. London, 1865, 8vo, pp. 203.

THIS book, of which the author was snatched away by death at the early age of 23, well deserves its modest title, but has in reality more pretensions than merely to assist the study of ovarian disease, for it enters fully into the subject, and contains almost all that has been or is known about it up to the present time. The work commences with an interesting historical account of ovarian physiology and pathology, from remote times down to 1844; and, after paying a respectful and well-deserved compliment to his father, by republishing *in extenso* his contributions to the physiology of the human ovary, formerly published in the 'London Medical Gazette,' the author gives another historical notice of what has been done by various modern investigators from 1846 to 1864, and concludes with a chapter giving his own views of cyst-formation in the ovary. The few remarks we shall offer to our readers will be with reference to this last chapter only, and perhaps we can scarcely do better than quote a few passages in order to represent some of the views of the author. He says—

"The normal function of the ovary is to produce cysts. It is formed composed entirely of cysts in different stages of development, and containing within them germs which themselves may be converted into secondary cysts. From the very earliest infancy, while still the fœtus is in its mother's womb, these cysts are being formed, becoming perfect and passing away. This series of operations is ever recurring, and ever being modified. The highest stage to which a cyst attains may be a simple globule, or it may be a fœtus and a corpus between. It may perish unnoticed, or it may powerfully affect the whole system, and end in the production of a new animal. Between these two extremes there are innumerable stages, innumerable halting places at which the cyst development may stand still, or may deviate from the physiological course, and become altered by such external agencies as pressure and inflammation. Adventitious structures are few and far between. There are no adventitious structures peculiar to the ovary. Tubercle or cancer may be developed, hydatids may be found, but these are of comparatively rare occurrence. The great mass of ovarian disease is due to slight errors of nutrition. More serum is poured into a vesicle than is normal. The secretion is retained instead of being got rid of, or is poured into a cavity which was not intended to receive it. Cells go on multiplying *ad infinitum*, and thus produce hyperplastic growths. Changes which normally occur in the contents of vesicles after they have been discharged take place within the vesicles themselves, either in perfectly regular series or slightly modified by their novel situation. Such are the different ways in which ovarian cysts are produced."

The following is Dr. Ritchie's description of the mode in which a Graafian follicle is converted into a pathological cyst:

"Let there be a slight increase in the strength of the Graafian wall, or a slight diminution of the eccentric force necessary to burst it, and the follicle immediately becomes a cyst. Its nutrition is now altered: the ovum disappears; the walls become thicker, tougher, and more compact; the diameter of the follicle increases as the serum collects in its cavity. As the cyst enlarges it presses upon the surrounding parts, and may even cause absorption of the ovary from which it sprang. More frequently, however, this does not take place. Usually the cyst, before it has attained any great size, has so much overtopped and protruded from the ovarian surface that it exercises little or no pressure upon it, and the ovary remains attached to the cyst. It is not, however, very frequently the case that one Graafian follicle alone becomes dropsical while the rest remain healthy. It is more usual for many follicles to enlarge simultaneously, the same causes acting upon all. The different cysts thus formed meet each other, are pressed together, and adhere. The pressure increases; the partition wall becomes thin, ulcerates, and throws the cysts into one general cavity. Those cysts which are least acted upon by pressure become the largest, and thus various forms of multilocular tremor are produced."

Having thus given a specimen of the author's description of, and reasoning upon, a very interesting pathological subject, we conclude our notice by recommending the work as a fair synopsis of what is at present known on the subject of ovarian physiology and pathology.

Art. XI.—*Des Maladies Mentales et des Asiles d'Aliénés.* Par J. P. Falret, Médecin de l'Hospice de la Salpêtrière, &c. Avec un Plan de l'Asile d'Illenan. *Paris:* pp. lxix, and pp. 797.

This is a bulky volume on Insanity, by the elder Falret, whose name has been prominent in the ranks of psychologists for nearly half a century as a clinical teacher and a writer of various essays on psychiatry. In 1822 he published his well-known treatise on suicide and hydrophobia, which justly achieved for him a reputation as a diligent observer and sound thinker. At that early period in his career it was his ambition to produce a complete work on mental disorders, but, as he tells us, it has not been his good fortune to be able to realise this project. To this failure, coupled with the desire to leave some monument of his labours behind him, we owe the appearance of the present book, which is nothing more than a collection of the principal contributions of the author, in the form of lectures and essays that have been delivered or published from time to time during his long career as an hospital physician. Consequently, there is a want of homogeneity in the work, and, at the same time, considerable repetition.

Sensible of the inadequacy of such a work, of which the different sections have been written at various times and under varying circumstances—as touching his own experience and his knowledge of the teachings of others—to afford a coherent history of insanity and to represent the results of his matured experience, he has prefaced it by a lengthened introduction, explanatory of his present opinions. This introduction, consequently, forms the most interesting portion of the book, as being the only novel matter in it. By its composition he has, he remarks, sought to establish unity of doctrine among the several contributions now collected together, and has endeavoured to portray the course of development of his scientific opinions and of his conclusions on the pathology and treatment of mental disorders.

After several years' research into the morbid anatomy of insanity, and after failing to elucidate the nature of the malady by the aid of the facts so gained, he informs us that he next attempted to interpret the nature of mental disorders by recourse to the doctrines of psychology, especially those of the Scottish metaphysicians. He minutely studied the modifications of the several faculties of the mind as exhibited among the insane, and persevered with his psychological inquiries for fifteen years, but at the end of that time had to lament that his labour was all vanity and vexation of spirit. The application of psychology to explain the phenomena of insanity is now, in his opinion, a practical mistake, and prejudicial to the real progress of science. Instead of supplying a useful grouping of symptoms according to the features of the malady, this psychological basis of investigation restricts the observer to a work of abstraction, gratifying to the mind like an ingenious trick, but without practical utility. Moreover, it fails not only in regard to semeiology, but also in relation to etiology, pathology, and therapeutics; and, as a general fact, by its introduction into the study of mental medicine, the essential nature of the disease is thrust out of sight and only symptoms are kept in view. On the other hand, it is by clinical and personal study alone of the insane that the physician can hope to discover the basis of his special science.

Falret recognises the duality of human nature, and that all intellectual and moral action requires the co-operation of the brain. But cerebral function he supposes to be governed by special laws, distinct from those that regulate the other functions of the body; and he hazards a theory of what he terms "psychical resultant," implying that in the incessant movements of the mind going on within itself, the first resultant becomes the cause of fresh effects; in a secondary, tertiary, &c., series. The hypothesis of human duality and of an intermediate faculty is considered by the author to reconcile the conflicting views of somatists and spiritualists regarding the nature and varieties of mental derangement. He, morever, admits a primary organic change of cerebral matter to be necessary, yet only as a condition furnishing a general aptitude for delirium, and not as one explanatory of its multiple forms and of their mode of production.

To render observation of the insane complete it is not enough to draw up a history of their disordered ideas in general, but it is requisite to study the history of individual cases. The insane condition is characterised much more by contrast with that of sane individuals under similar circumstances, than by any positive manifestations of disordered mind. The emotional characteristics are of primary importance in the investigation, for, as a rule, the first changes are discoverable in the emotions and affections, and appear to constitute the foundation for the development of intellectual delirium and to mark the period of incubation or elaboration.

Madness is not a single morbid entity, but presents itself under the most varied forms, changeable without limit according to individual peculiarities and accidental circumstances. Neither is there unity in the delirium of mental disorder; no delirium is limited to one idea or to one series of ideas; and hence monomania, as defined by systematic writers, has no real existence. This problem of the existence of monomania constitutes the topic of one of the essays collected in the present work, and is largely discussed both in its clinical and psychological aspects.

In the above sketch we have alluded to some of the doctrines propounded by Falret in the introduction on certain questions concerning mental maladies. On all such questions he has many original remarks, and fails not to interest, if, perchance, he does not convince, the reader of the truthfulness of the conclusions he asserts. His opinions are clearly stated and sturdily upheld; and when regarded as those of a highly instructed man, developed in the course of a very wide experience, prolonged over a space of nearly fifty years, they have a high claim upon our thoughtful study and consideration.

We should, indeed, have preferred a well-matured and homogeneous, if much briefer, work than the volume before us, from which we might have more fully culled those practical lessons which such long and wide observation must have taught the author. It would have formed a much more satisfactory monument to his fame, and have survived him a longer period than can be anticipated for this collection of productions of bygone years.

To render some account of the contents, suffice it to state that the first two chapters are occupied with general considerations on the nature, course, symptoms, and treatment of insanity; that the ten lectures printed next in order treat of the observation of the insane, of the phenomena of insanity as exhibited in the emotions and affections, and in the intellectual functions; of illusions and hallucinations; of disorders of physical sensibility and movement, and of the course and terminations of mental disorder; that the succeeding chapters are severally occupied with the subject of delirium; of the non-existence of monomania; of the existence of mania without lesion of the intellect; of apoplectiform congestion and epilepsy; of the clinical teaching of insanity, its desirability and the objections brought against it; of a notice of visits to the Illenau Asylum, in Baden; of the general treatment of the insane; of the utility of schools and of reunions in the treatment of the insane; and, lastly, of the scope of the lunacy legislation for France, in 1837, and the measures needed. The concluding section consists of the discourse delivered by Falret at the tomb of Esquirol in 1840.

This outline of the contents and the remarks made on the general character of this book on mental maladies will prove to the reader of this notice that by becoming possessor of the work at the small cost of ten shillings he will acquire a valuable addition to his library.

ART. XII.—*Orthopraxy. The Mechanical Treatment of Deformities, Debilities, and Deficiencies of the Human Frame.* By HENRY HEATHER BIGG. *London:* Churchill. 1865. Pp. 709.

MR. BIGG has already published several works on surgical mechanism; all of which are, as it appears, collected into this volume, which contains a description of the appliances recommended by the author in all the various deformities, whether the result of accident or disease, which occur in ordinary surgical practice. We notice with pleasure that Mr. Bigg does not confine himself to his own inventions, but describes those of his predecessors as mechanicians, as well as many which have been devised by surgeons, and speaks of them with apparent fairness and discrimination. The book, in spite of its strange and somewhat quackish name, and in spite of a rather pretentious tone, is instructive and useful, and, as far as we are able to judge, well calculated to assist the practitioner in many of the exigencies which he finds most troublesome. There is no question that medical men in general do not understand the principles of mechanics, still less their application in the construction of instruments. Hence the utility of a class of well-instructed instrument-makers who can supply this deficiency; and hence, also, the usefulness of such books as this of Mr. Bigg. But there can be equally little question that instrument-makers do not understand anatomy or the principles of surgery, and still less the application of those principles to practice. Whether Mr. Bigg may have been more initiated into these matters than his fellows we need not stop to inquire; but however that may be, and however necessary the instrument-maker's assistance may be in a difficult case of deformity, congenital or acquired, the guidance and discretion of the treatment must always be with the surgeon, to whom the instrument-maker is only an assistant subordinate. This fact Mr. Bigg seems rather disposed to forget. For instance, he speaks (on page 680) of a young lady who "was placed under *my* care for equino-valgus." "The history of the varied treatment to which this case had been subjected for many years almost deterred me from undertaking its management." "I resolved, however, to make another attempt." "In the first place, I consulted my friend Mr. Nunn upon the propriety of dividing the tendo-Achillis. He agreed with me on the necessity for this step and performed the operation," &c., &c. Surely this is an inversion of the natural order of things. The young lady ought to have been under the care of the surgeon, who might, no doubt, have advantageously told her to buy a boot of Mr. Bigg. It behoves surgeons to look into this matter, not less for their patients' interest than their own. We need not dwell on the undignified position which a surgeon occupies when he turns tendon-cutter in the employ of an instrument-maker; but we would rather ask whether the patient would be likely to profit either in purse or person by being placed in the hands of those who, after all, have no knowledge whatever of surgical matters? We do not make these observations in disparagement of Mr. Bigg, who may, for aught we know, be as well qualified to conduct a surgical case as Mr. Nunn or any other surgeon; but, as a general principle, a man must choose one thing or the other. If he is an instrument-maker, he should not be a surgeon, nor should surgeons abet him in the endeavour to pass for one.

Art. XIII.—*Dictionary of Science, Literature, and Art.* Edited by W. T. Brande, D.C.L., &c., and the Rev. G. W. Cox, M.A., &c. *London:* 8vo, Parts IV to VIII, 1865.

A notice of the first three parts of this Dictionary appeared in the July (1865) number of this Review; since then, four other parts have been published, and the letter M in the alphabetical list of articles reached. A monthly issue was promised, but no part has appeared since November last; a prolonged intermission prevalent among publications when brought out into parts, and a cause of vexation and disappointment among the subscribers. In the present instance some excuse may be found in the death of one of its editors, the much lamented Professor Brande, to whom, indeed, belongs the merit of having originated the Dictionary, some twenty-five years ago. The loss of such an editor will with difficulty be replaced, for we presume that Mr. Cox will not continue without the co-operation of a co-editor whose reputation will offer a guarantee for the character of the scientific articles contained in this important work.

In our previous notice we were compelled to animadvert upon several inaccuracies and defects of the definitions and histories of not a few of the objects described, particularly among those belonging to physiological, medical, and microscopical science; and we then expressed a hope that more consideration might be bestowed upon such matters. We, however, regret to state that many articles in the four numbers last published are equally defective with those previously cited as unsatisfactory. As medical critics, we naturally turn to subjects connected with medicine and the allied sciences; and lest we should be considered to travel beyond our scope, we restrict our criticisms to those subjects.

We find diseases to be generally described in such a summary manner that even when the account is clear and accurate it conveys no satisfactory information. The directions for treatment are still more open to objections; they are not only deficient, but also belong to an old-world style of therapeutics, and would be mischievous if followed. We must repeat what was said in the previous notice of this Dictionary, that medical terms might advantageously be omitted, and that the treatment of diseases certainly should find no place in its pages. Anatomical and physiological subjects fare little better, as regards the descriptions given of them, than do medical. In frequent places the writing is slovenly and errors creep in. The account of Infusoria is more than twenty years out of date, being borrowed from Ehrenberg's large work published in 1838; the notice of heat, of endosmose and exosmose, is very meager; the history of intestinal worms not brought down to the present day, and the descriptions of the structure of the larynx, liver, and lungs, worthless.

We might add largely to this list of unsatisfactory articles, but will forbear doing so, and confine ourselves to quoting an extract or two to substantiate the justice of our criticism: "The tympanum separates (?) the external from the internal ear; it is closed by a membrane called the *membrana tympani* or drum of the ear, upon the inner surface of which a nerve called the *chorda tympani* ramifies (?)." Again, we are informed that "the vestibule is a small cavity in the petrous portion of the temporal bone, having a little spiral cavity called the *cochlea* connected with it, and three semicircular cavities or tubes bent in a semicircular form;" whereas two of the semicircular canals are vertical and one horizontal. No mention is made of the existence of the Eustachian tube. Similar looseness and inaccuracy of detail are found in the description of the eye. For instance, we read—

> "The conjunctive membrane of the eye, called also tunica albuginea, or white of the eye (!), is a membrane which lines the inner eyelids and the fore part of the globe of the eye. The internal parts of the eye are: the *sclerotic membrane*, which is the hard outer case of the globe; the *choroid membrane*, which is the interior coat of the sclerotic, beginning around the optic nerve, and proceeding to the margin of the transparent cornea, where it deflects inwardly, forming the *iris*, the posterior surface of which is called the *urea* and its central opening the *pupil*, which is muscular. . . . The *crystalline lens* is a pellucid body, included in a delicate *capsule*, and lodged in a concave depression of the front of the *vitreous humour*," &c.

Judging from this confused and inaccurate sketch of the structure of the eye, it must have been the work of an individual having no anatomical knowledge, or otherwise of one destitute of the power of concisely and intelligibly reducing his knowledge to writing.

ART. XIV.—*The United States' Sanitary Commission. A Sketch of its Purposes and Work.* Compiled from Documents and Private Papers. Published by Permission. *Boston.* 1863. Pp. 299.

IN this little book we have a history of the Sanitary Commission from its origin, showing how the first idea of it was started by the patriotic women of the Northern States; how, from a small beginning, it rapidly grew into a great institution, and during the continuance of the war increased its exertions with the increasing wants of the armies, affording the most efficient aid to the Government in relieving the various and great necessities which so vast and protracted a struggle engendered.

The publication is anonymous. We are informed that it did not originate with the United States' Sanitary Commission, nor with any of its officers; but is written by one who has served with the Commission from the first, and who may claim to comprehend its purposes and its work, and to relate its facts with accuracy; and, further, that the entire profits of the book are for the treasury of the Commission.

We could have wished that the name of the author had not been kept back; he has performed his part so well, in an impressive, often eloquent manner—in a manner, in brief, worthy of his great theme. His narration for most part is very ample; as the date of the work shows, it is not brought up to the termination of the great war, but only to its turning point—the autumn of 1863—when hope of success had become the prevailing feeling in the Federal States and had cheered them on to greater exertions.

Having in preceding numbers of our Review taken notice of this noble institution from its beginning, and described in some detail its organization, it is the less necessary to enter into particulars respecting its system as laid before us in these pages. We have read the book with infinite satisfaction, and we must express the hope that it will find in this country very many readers. We know no work in which in so limited a space the incidents of war are so strikingly delineated, and in which the scenes of war are brought in all their horrors more vividly to the mind; happily, however, not sparingly relieved by deeds of mercy and heroism all but compensating, and by traits of tenderness, and sometimes of comic humour, as unexpected as gleams of sunshine in a storm.

Besides general readers, we could wish that this book formed a part of every military library and was in the possession of every medical officer, there is so much in it that is instructive and demonstrative both as regards the wants of armies in the field in a protracted campaign, and the means of meeting and relieving them.

Should it fall into the hands of our countrywomen we are sure they will be deeply interested in it, the women of the United States having performed their parts so admirably, following the example set them by Englishwomen in the Crimean war—that war specially remarkable for its disasters owing to bad management and ignorance of sanitary science. We are informed that the supplies in dresses, comforts, &c., furnished by the women and sent to the stores of the Commission, in money value reached the enormous amount of seven millions of dollars. But they did more than contribute from their homes, many of them attended the hospitals and proved efficient and tender nurses. There is a narrative by a lady of her experience at and after the battle of Gettysburgh, which can hardly be read but with a moistened eye, so pathetic is it and so simple. Many and strong were the expressions of gratitude which the women received from the soldiers whom they nursed. Many extracts from letters are given in proof, and with them the following lines addressed to a lady-nurse by a private of the 16th Regiment New York Volunteers, introduced with the remark—"Surely no lady has ever received a more graceful acknowledgment of kindness"—

"From old St. Paul till now,
Of honorable women not a few
Have left their golden ease, in love to do
The saintly work which Christ-like hearts pursue;
And such an one art thou! God's fair apostle,
Bearing His love in war's horrific train:
Thy blessed feet follow its ghastly pain,
And misery and death, without disdain.
To one borne from the sullen battle's roar,
Dearer the greeting of thy gentle eyes,
When he is weary, torn and bleeding lies,
Than all the glory that the victors prize.
When peace shall come, and homes shall smile again,
A thousand soldiers' hearts in northern climes
Shall tell their little children in their rhymes,
Of the sweet saint who blessed the old war times."

On the Chickahominy,
June 12*th*, 1862.

We are tempted to make another extract; it relates to discharged and disabled men, and is worthy of serious attention in this country. The sentiments expressed in it are highly creditable to the author and to the American people on the supposition that his view is a popular view; and that it is such, may, we think, be inferred from the remarkable fact that, in a few months from the termination of the war, the vast armies which were in action have for the most part been disbanded, and the men who composed them, from the general highest in command to the private soldier, have returned to their homes and have resumed either their former or new occupations in civil life. What the author insists on is offered suggestively in four propositions. The third and fourth, as most to our purpose, we quote—

> "To avoid the danger of interfering with natural laws—a thing not to be tolerated in our young and healthy country—by any scheme of herding the invalids of war in public institutions. Such schemes would strike a blow at domestic order and the sacredness of home affections, whilst they would take from the soldier that spirit of independence which is his birth-right and his safety. We do not want a vast network of soldiers' poor-houses scattered through the land, in which these brave fellows will languish away dull, idle, and wretched lives. But we want an endeavour to promote the healthy absorption of the invalid class into their own homes, and into the ordinary industry of the country, there to live and labour according to their remaining strength, sustained, honoured, and blessed by their own kindred and community."

ART. XV.—*Report on the Physiological Action of the Nitrite of Amyl.* By B. W. RICHARDSON, M.A., M.D.[1] Pp. 10.

ONE of the most useful functions of the British Association for the Advancement of Science, is the making of grants of money in aid of special researches. Besides the pecuniary assistance, the circumstance that the results will be communicated *vivâ voce* before an audience competent to judge of their value, must be a stimulus on the part of the recipient to exert himself, and bring the inquiry he has pledged himself to engage in to a successful result. The paper before us is an example in point, and a happy example; the facts which are described in it being as new as they are interesting. And we are glad to know that, similarly encouraged by additional grants, its zealous and able author is prosecuting his researches further in the examination of the amyl compounds.

Nitrite of amyl (the subject of this report) is an inflammable fluid, of sp. gr. ·913, boiling at 182° Fahr. Its odour is that of over-ripe pears; its composition $C_{10}\ H_{11}\ NO_3 + HO$: an antiseptic, its vapour extinguishes phosphorus. Its physiological properties are those which are most peculiar and remarkable. The following is a summary of them, as stated by Dr. Richardson, after a detailed account of the various experiments by which they were ascertained.

"1. It is absorbed by the bodies of animals however introduced into the organism—by the skin, by the stomach, by the lungs, by the cellular tissue.

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[1] From the 'Report of the British Association for the Advancement of Science,' for 1864.

"2. After its absorption its effects are seen immediately on the heart and circulation; there is in the first instance violent action of the heart, with dilatation of the capillaries, followed by diminished but not extinguished power of the heart and contraction of the extreme vessels. As an excitant of vascular action the nitrite of amyl may be considered the most powerful agent as yet physiologically discovered.

"3. On animals, such as frogs, whose bodies admit of its removal spontaneously, and whose circulatory and respiratory systems are simple, the nitrite suspends animation, and when the animals are placed under favorable conditions for the process of recovery they may recover after considerable periods of time. There is no other known substance that suspends animation in these animals for so long a period. On warm-blooded animals, which are clothed in thick and less penetrable skins, and in whose bodies the circulatory and respiratory systems are more complicated, the nitrite cannot actually stop the movements of respiration and circulation without destroying life. But even in these animals it can, without destroying life, reduce the forces of respiration and circulation so extremely that a condition precisely analogous to what is known as *trance* or *catalepsy* in the human subject can be brought on and sustained for many hours.

"4. The nitrite of amyl is not anæsthetic: by it consciousness is never destroyed, unless a condition approaching to death be produced.

"5. The effects of the nitrite on the organism are directed to the motive force, which it first wildly excites and then subdues.

"6. The *modus operandi* of the nitrite appears to be by arresting the process of oxidation in the tissues.

"7. Physically the nitrite holds a place between the volatile bodies, such as chloroform, and the solid bodies, such as opium and woorali. Hence its effects are less evanescent than those of the very volatile substances, and less certainly destructive than the solid substances. In this lies the secret of its prolonged action."

The pathological effects of the nitrite remain to be ascertained. The author expresses a hope that it may form an efficient excitant in cases of syncope, by rousing the heart's action, and also in tetanus, "by paralysing the extreme filaments of nerves and reducing the muscular power of all the voluntary muscles in the same manner as woorali."

He enters into a curious speculation, founded on the effects of the nitrite in suspending animation in frogs, whether it may not have the like effect on man, in individuals of a peculiar idiosyncrasy, throwing them into a state of trance or catalepsy, or a state of apparent death without its reality; such, for instance, as certain Indian fakirs are believed by many to be capable of assuming, and in whom, he conjectures, a chemical substance may be formed, "which, without actually stopping the motions of the heart and respiration, suspends them so nearly, that passive life only is carried on, and that this condition is continued until such time as the substance is removed from the circulation." This is ingenious; but before reasoning on and attempting the explanation of such marvels, we could wish to have them established as to their truth, beyond question, by the strictest scrutiny. It was long believed that divers could gradually acquire the power of suspending respiration for a long time, as Lord Bacon has it, by practice "to have brought themselves to hold their breath an incredible while," and yet, when put to the proof, as in the instances of the pearl-divers of Ceylon, none were found capable of remaining under water with impunity beyond a minute and a few seconds. Could a human being be found with an hybernating property, and the fakir exploit be well authenticated, the mystery would perhaps vanish. We need hardly remark, that the credulity of mankind is enormous, and the deceptive power of man, under the entanglement of credulity, is commensurate.

ART. XVI.—*On the Detection of Aconite by its Physiological Action; being Notes of Experiments made in connection with the trial of Dr. E. W. Pritchard.* By FREDERICK PENNY, F.R.S.E., &c., and JAMES ADAMS, M.D., F.F.P.S., Glasgow. —*Glasgow*, 1865. pp. 28.

2. *On the Application of Physiological Tests for certain Organic Poisons, and especially Digitaline.* By C. HILTON FAGGE, M.D., and THOMAS STEPHENSON, M.D.—*London*, 1865. pp. 5.

IT is interesting and consolatory to find that with the advance of science, the means for the detection of crime increase. Of this we had satisfactory proof, in the instance of atrocious murder by poisoning, perpetrated by the late Dr. E. W. Pritchard, of Glasgow. The researches in quest of the suspected poison or poisons were, as conducted by Mr. Penny and Dr. Adams, of two kinds, one chemical and the other physiological.

The following extract gives the conclusions which Mr. Penny, whose evidence was given in court, considered himself warranted in drawing, from the results of the examination of the parts taken from the body of Mrs. Pritchard; like results were obtained, leading to like inferences, from the examination of the body of Mrs. Taylor:

"1st. That all the parts of the body examined by me contained antimony. 2nd. That in the dried contents of the intestines the antimony was partly in a form soluble in water, and most likely in the state of tartar emetic or tartarised antimony. In the liver, kidney, and other viscera, the antimony was deposited in a state insoluble in water. 3rd. That the contents of the intestines contained the largest proportion of antimony, next the heart, then the liver, kidney, and spleen; less in the stomach, and the smallest quantity in the rectum, brain, and blood. Not knowing the total weight either of the contents of the intestines or of the several organs here enumerated, I was unable to calculate the total quantity of antimony in these matters, either separately or conjoined. 4th. That the contents of the intestines, the spleen, the heart, the blood, and the kidney contained mercury; but that none of this metal was present in the liver, stomach, rectum, and brain. That in all these matters the mercury was in a state insoluble in water; and this result is quite consistent with the known property of mercury to form insoluble combinations with animal substances, even though it had been taken or administered in a soluble form during life. 5th. That the largest quantity of mercury was contained in the contents of the intestines, next in the spleen and heart, and extremely minute traces in the blood and kidney. 6th. That the presence of antimony and mercury in the contents of the intestines indicates that these metals were being passed from the deceased up to the time of death. 7th. That no other metallic poison was contained in the matters examined. 8th. That no aconite, morphia, or other vegetable poison discoverable by chemical processes was contained in the contents of the intestines or in the stomach. 9th. Not having detected any organic poison, either in the said contents of the intestines or in the stomach, it was not necessary to examine the other articles for such poisons, and more especially as the quantities of these matters received for analysis were too small to hold out any prospect of successful results."—Signed with this solemn declaration, "All this I certify on soul and conscience."

The physiological inquiry was made on the principle, that the substance or mixture suspected, if it contain a poison, will, when given to an animal, produce the effects which that poison is known to have on the same animal. The subject for trial was a portion of a preparation of opium—Battley's solution—which the mother-in-law, Mrs. Taylor, had been in the habit of using. The first indication it afforded was, when tasted, and applied to the tip of the tongue in the form of a soft extract, to which it was reduced by evaporation, the production of a sensation different from that which was experienced from the genuine; "thus there was felt, quickly supervening upon the taste of *genuine* Battley's solution, a peculiar benumbing and tingling sensation, increasing in intensity for a short time, and persisting for some hours."

The animal selected for comparative trial was the rabbit; the method, epidermic injection; the results were strongly marked. The genuine Battley's occasioned relaxation and stupor, but without any convulsive motions; the suspected produced strong convulsive movements, and there were differences in the other resulting symptoms. So well marked, indeed, were the comparative results, that the authors came with confidence to the conclusion, that aconite was present, and in a notable proportion, in the suspected solution, which was confirmed, as they state, " by the facts of our finding no effects analogous to those of aconite produced by any of the agents (veratria, strychnia, &c.) employed by us in the concurrent experiments, or by any other poisonous agent with the action of which we were familiar." And that their confidence was not misplaced, was shown by the confession of the criminal, viz., that he had put aconite into the Battley's solution belonging to his mother-in-law.

The researches described in the second paper which we have to notice, that of Drs. Fagge and Stevenson, were conducted on the same principle as the preceding. The subjects which they selected for their physiological experiments were frogs, which they preferred to any other animal, for very sufficient reasons, such as they assign. The following are some of the conclusions at which they have arrived:

> " 1. Digitaline is one of a small class of substances of which the action on frogs appears to be identical. As the heart is the organ primarily affected by them they may be called cardiac poisons, so far as frogs are concerned.
>
> " 2. These substances are, besides digitaline, the *Upas antiar*, the *Helleborus viridis*, and perhaps other species of *Helleborus*, the *Tanghinia venenifera*, the *Dajaksch* or arrow-poison of Borneo, the *Carroval* and *Vao*, South American arrow-poisons and the *Scilla maritima*. Of these we have ourselves experimented only with digitaline, antiar, the *Helleborus viridis* and the *H. niger*, and the *Scilla*, and we believe that we are the first observers who have recognised the identity of the action on frogs of the last of these plants with that of the other two substances placed in this group. Besides digitaline only two of them, namely, *Helleborus* and the *Scilla*, are likely to be the subject of medico-legal investigation in this country, and that but rarely.[1]
>
> " 3. The characteristic effect of each of these agents on frogs is the production of irregularity of the heart's action, followed by complete stoppage of its pulsations, the ventricle remaining rigidly contracted and perfectly pale, after it has ceased to beat; the muscular

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[1] They make an addition in a note to the list of "Cardiac Poisons," that of the Manganja, or arrow-poison brought by Dr. Kirk of the Zambesi Expedition, tried first by Dr. Sharpey and afterwards by themselves.

power of the animal being at this time unimpaired, and persisting as long as in frogs in which the circulation has been stopped by other means, such as a ligature of the heart."

The conclusions following these, as many as ten, we need not quote, as the paper is published in the 'Proceedings of the Royal Society' (the number for May, 1865), which are so accessible; but we must not omit mention of one of their most remarkable results, viz., that the contents of the human stomach had, in almost every instance in which they made trial of them, a poisonous effect on the lower animals, leading to this important inference—

"That unless some points of difference should hereafter be discovered it will render impossible the detection of many vegetable substances (among which they mention lobelia, emetina, veratrum viride, and delphinium staphisagria), by their physiological effects."

And, they add,—

"It makes invalid (at least as far as frogs are concerned) all evidence of this kind, in which the state of the heart is not more particularly described than has hitherto been the case."

What words of warning should these be to those who are engaged in medico-legal inquiries bearing on criminal cases! Whilst certain positive results, well ascertained, seem to justify the use of the physiological test in these researches; others, such as the above mentioned, enforce at least the greatest care in conducting the trials, and the greatest caution in making deductions from the results.

We need hardly remark, that one promising advantage of these researches, in addition to the aid they may afford to the administration of justice and the detection of crime, is, that they may enlighten us as to the true action of some of the most powerful medicines, all those which are poisonous in non-medicinal doses, whether of the inorganic or organic kind. In exemplification, though this is hardly required, and in proof of the need of extreme caution, we may mention that Mr. Penny and Dr. Adams, from their experiments, came to the belief that the profession at large are labouring under an error in relation to the action of opium, viz., that convulsions are one of its characteristic effects on mammiferous animals. They say, "We have satisfied ourselves that, as regards rabbits, no convulsive seizures attend the action of opium." Their experiments were made with extracts obtained from Battley's solution and laudanum, and were limited to rabbits. Their conclusions would have been more convincing had their experiments been extended to other animals of the mammalian class, especially dogs, on which Orfila experimented, and with results decidedly opposed to those obtained by these gentlemen.[1]

ART. XVII.—*What Food to Eat.* By W. W. IRELAND, M.D. (Late of H.M. Bengal Army). *London,* 1865. pp. 52.

THIS, a logically reasoned and pleasantly written pamphlet, is well adapted to enlighten those persons who have hitherto paid but little attention to the interesting subject which its author discusses. The tenor of his arguments is to prove that whilst a diet consisting principally of animal food is most stimulating, that of which vegetable food forms the larger proportion, is more sustaining, and is apt to produce chyme or blood less prone to decomposition; and further, and most important, that a vegetable diet, well selected, is adequate to the support of average health and strength and intellectual development, and consequently may well supply the place of a mixed diet for common use amongst the mass of the people who cannot afford to purchase the higher-priced butcher's meat.

From his experience in India, he is able to point out many fallacies, which even physiologists of no mean repute have fallen into relative to the diet of the several races of that vast country, tending to confirm the conclusion that animal food is nowise essential to health or vigour, bodily or mental. We quote the following passage, as clearly expressing the opinion the author has arrived at on the important matter of food. He prefaces it with the remark, that he is induced to offer it from his paper having been necessarily aggressive, striving to keep a middle ground between a too generally received fallacy and the extreme views of the Vegetarians.

"He (he says) believes that the Creator of the Universe has placed within reach of almost all the tribes inhabiting this globe food sufficient to maintain them in full health. The hunter has to take an unusual amount of exercise to obtain a living, and his food is peculiarly suited, not only to support this exercise, but to render it agreeable. The food of the herdsman is also suitable to his wants and nature; and the dietary of the civilised nations is equally fitted for the multifarious exigencies of their complicated life. These may be met and supplied either upon a diet containing a measured proportion of flesh and vegetables, or upon a well-chosen vegetable diet. This is a conclusion which presents no difficulty to our acceptance, since both vegetable and animal food have been proved by chemists to contain the same nourishing principles. There is no necessity why people who can afford it should be deprived of the delicate juices and sometimes useful stimulus of flesh meats; but, as already said, the comparative influence of a mixed diet over a vegetable one is not great enough to make it a prominent cause of superiority. We cannot, with the means at present in our power, prove the superiority of one diet over the other, for men have done the same work equally well upon either."

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[1] See his 'Toxicologie Generale,' partie iii, p. 129.

Referring to the cattle disease, now so alarming, he expresses a hope, that as the potato disease led the people of Ireland to use a better and more mixed dietary, so this cattle plague may have induced the English to alter their views relative to the value of meat.

Of all vegetables the leguminosæ possess the highest proportion of flesh-forming principles. Wheaten bread contains more nearly than any other substance the proportion of plastic and respiratory material necessary for the wants of the body in our climate, and hence has been justly called the "staff of life," being capable of sustaining life almost without any addition. The leguminosæ, on the contrary, such as peas, beans, and especially Indian dhal, are so rich in flesh-forming principles, that like meat, they need to be used with substances less abounding in nitrogen, more in carbon, such as rice, potatoes, oil, or butter. On the last mentioned, dhal, Dr. Ireland lays much stress for its excellent qualities: we quote what he says, as we learn that it is to be had in this country, and at the same price as rice. "It has this advantage over other leguminous products, such as peas and beans, that it is very palatable. It requires to be ground and allowed to simmer, butter being added, it is then well peppered and eaten, mixed with boiled rice. This is what in India is called 'kichery,' and no substance is more capable of supplying the want of flesh meat. I have heard a Hindu say, 'Dhal is my flesh.'"

Our author writes in a very happy manner relative to the propriety of variety of diet: he says—

"How clever nature is! how suitable are her rules, and how multifarious her results! She takes a few elements, nitrogen, carbon, oxygen, hydrogen, and sulphur, combines them together under the influences of sunlight, and produces a few compounds fit to be assimilated by animals. These substances approach one another very nearly in chemical composition, yet are often widely different in feature and appearance. She flavours them in an immense number of ways with a few aromas and essences. This diversity of taste is

> not only pleasing to the palate, but also useful to the health. It has been proved by the statistics of twenty hospitals that when one monotonous diet-roll is maintained the food becomes insipid, and at last loathsome to the taste, and that the health of the inmates is always materially improved by a change of dishes. No one or two kinds of food, therefore, can be held up as really better than the others. Man's constitution demands variety, and this variety nature has bountifully afforded."

We have prolonged our notice of this little book more than we had intended, drawn on partly by the vast importance of the subject, especially at the present time; partly by the interesting and able manner in which it has been treated. In relation to the former, we indulge in an additional extract. It is of an encouraging kind, and in harmony with views often taken in former numbers of our 'Review.'

> "For my part (he says) far from believing, like a well-known professor, that the rise of butchers' meat a halfpenny the pound would increase our national mortality, I believe that we could lack all the meat which is lost in the cattle plague, and yet that if we replaced it by a *well-chosen* vegetable diet, while the price of meat would remain nearly stationary, the national health would improve. For, vegetable food not being so tempting, we should get rid of much of the over-eating in which many people indulge, and a great deal of money would be saved which might be more usefully spent in other things."

One more quotation we must give for the sake of our children, who, we believe, amongst the rich, are often fed too much on animal food.

> "A distinguished philosopher, Mr. Herbert Spencer, in his work on education, laying down the axiom that children require more nourishing food than adults, would, contrary to the received opinion, commence giving them flesh at two or three years of age. He says: —'This relatively greater need for nutriment being admitted, as it must be, the question that remains is, Shall we meet it by giving an excessive quantity of what may be called dilute food, or a more moderate quantity of concentrated food? The nutriment obtainable from a given weight of meat is obtainable only from a larger weight of bread, or from a still larger weight of potatoes, and so as to fulfil the requirement, the quantity must be increased as the nutritiveness is diminished. Shall we, then, respond to the extra wants of the growing child by giving an adequate quantity of food as good as that of adults? Or, regardless of the fact that its stomach has to dispose of a relatively larger quantity even of this good food, shall we further tax it by giving an inferior food in still greater quantity?'"

To this fallacious reasoning our author justly replies, observing—

"Now the amount of flesh-forming nutriment is greater in beef than in bread, but the amount of respiratory food is too low, and consequently, though a child might exist upon bread alone, it could hardly do so on flesh. Moreover, the proportion of flesh-formers in beef is less than in various products of the leguminosæ. What, then, becomes of Mr. Herbert Spencer's argument? In France children are often fed upon flesh meats when very young, but they are not so healthy as English children, and their mortality is greater."

He very properly adds—

"Without fresh vegetables we should soon die of scurvy; and without a certain bulk to our food we should find that it did not agree with us."

May not the public be wisely warned against a diet mainly consisting of animal food? And ought they not to be advised to adopt a change, not indeed to resort to a total disuse of butchers' meat, but a reduced allowance of it; and this not only in the families of the rich and of the well-to-do—the poor, alas! need not be exhorted to such an economy—but also in our workhouses, prisons, and county lunatic asylums, in the dietaries of which animal food so largely enters, and, as we believe, so unnecessarily. There is ample proof that the change would be safe, and, as we believe, in ordinary times beneficial; indeed, we go so far as to think that in the institutions just mentioned, a dietary exclusively vegetable, properly selected, might be, unless during epidemic, substituted for the present one in use with perfect safety.

Whether the great loss of cattle and diminution of capital in the instance of the smaller farmers may not lead to the conversion of a good deal of grazing into arable land is a question it may not be easy to answer. Should it have such an effect, we are not those who would deplore it; we should witness what may be considered a compensating change, viz., with a decrease of production of animal food, an increase of vegetable food, an increased demand for agricultural labour, a less dependence on foreign supplies of grain, and, best of all, a diminished temptation on the part of farm labourers to emigrate.

ART. XVIII.—*Sanitary Commission, No.* 87. *Preliminary Reports of the Operations of the United States' Sanitary Commission in North Carolina, March,* 1865, *and upon the Physical Condition of the Exchanged Prisoners lately received at Wilmington, North Carolina.* Pp. 17.

2. *Narrative of Privations and Sufferings of United States' Officers and Soldiers while Prisoners of War in the hands of the Rebel Authorities. Being the Report of a Commission of Inquiry, appointed by the United States' Sanitary Commission. With an Appendix containing the Testimony. Boston,* 1864. Pp. 86.

THESE two papers contain a piteous account of the terrible privations to which the prisoners of the United States' Army were subjected during their confinement, and the sufferings they underwent from the inhuman treatment which they experienced. Were not the statements thus given to the public so well authenticated, we could not have believed them, the details are so horrible. The authorities which vouch for their accuracy are of the highest kind. First, we have an account of what Dr. Agnew and Dr. Dalton (the latter the well-known and distinguished professor of physiology) saw at Wilmington, where shortly before their arrival 2475 returned prisoners had been received. Secondly, we have the detailed report on the condition of the prisoners by a special commission, composed of six men of high character, three medical, three non-medical. The first were Dr. Valentine Mott, ex-president of the Medical Department of the University of New York, &c.; Dr. Edward Delafield, President of the College of Physicians and Surgeons of New York, &c.; Dr. Ellerslie Wallace, Professor of Obstetrics, &c., Jefferson Medical College, Philadelphia, &c. The second were Governor Morris Wilkins, Esq.; Hon. J. J. Clark Hane, Judge of the District Court of the city and county of Philadelphia; Rev. Treadwell Walden, Rector of St. Clement's Church, Philadelphia. It is right to mention, that all the oral evidence they collected was taken on oath.

We are informed by Drs. Agnew and Dalton, that had they not witnessed the sights which they described, they could hardly have imagined them; and we can truthfully say, that had we not read all the particulars as narrated both by these gentlemen, and in fuller detail by the special commission, we could not have had any adequate ideas of such a tragedy,—for that brief word is the only one applicable comprehensively to portray the whole of the treatment of, with its effects on, these unfortunate men.

The following is a concentrated picture of terrible misery from the pen of Dr. Agnew, written just after seeing the prisoners, and is confirmed with greater particularity by Dr. Dalton. It is an extract of a letter from him of March 20th, 1865, 11 p.m. We quote the whole, beginning with what General Abbott stated—

"The returned prisoners sent into Wilmington numbered nearly 9000. About 7000 of the less famished have gone north. General Abbott, who received our poor fellows in the exchange, has just told me that language would utterly fail to describe their condition. Filth, rags, nakedness, starvation, were personified. Many of the men were in a state of mind resembling idiocy, unable to tell their names, and lost to all sense of modesty, unconscious of their nakedness and personal condition. Some of them moved about on their hands and knees, unable to stand on their gangrenous feet, looking up like hungry dogs, beseeching the observer for a bite of bread or a sup of water. Some of them hitched along on their hands and buttocks, pushing gangrenous feet, literally reduced to bone and shreds, before them. Others leaned upon staves, and glared from sunken eyes through parchment-like slits of thin open eyelids into space, without having the power to fix an intelligent gaze upon passing objects. Others giggled and smirked and hobbled like starved idiots, while some adamantine figures walked erect, as though they meant to move the skeleton homewards, so long as vitality enough remained to enable them to do so. To see the men who remain here in hospital would move a heart as hard and cold as marble. Their condition is that of men who have for months suffered chronic starvation. Their arms and legs look like coarse reeds with bulbous joints. Their faces look as though a skilful taxidermist had drawn tanned skin over the bare skull, and then placed false eyes in the orbital cavities. They defy description. It would take a pen expert in the use of every term known to the naturalist and the physician to begin to expose their fearful condition."

In the narrative of the commission, which is divided into seven sections, and comprised in twenty-five closely printed pages, the particulars collected are all in accordance with the horrible picture of suffering quoted above, and is fully supported, first by the medical report drawn up by Dr. Wallace, in which he considers the causes in relation to the effects, and by the evidence of individual prisoners and others, taken on oath, —this in an Appendix occupying forty-two pages,—and further, by a Supplement of six pages, descriptive of the terrible condition of the great multitude of enlisted Federal soldiers—35,000 confined in the prison-camp of Andersonville, crowded within the limits of thirty-five acres, for most part without any shelter. We do not purpose to enter into minutiæ of their treatment; it may suffice to express an opinion from the evidence brought forward, that it was unjustifiable, barbarous, and most cruel. A very few extracts may, perhaps, best display this, and yet not thoroughly.

Lieut.-Colonel Farnworth, of the United States' army, who had been a prisoner, made solemn oath that the statements contained in a letter which he delivered is true. We quote only parts of it; it occupies three pages.

"Belle Isle.—Upon the 26th day of January, 1864, I visited Belle Island, as an assistant in the distribution of clothing sent by the government and by the Sanitary Commission of the north; this was my first time outside the prison walls in six months. The island is situated just opposite the Tredegar Iron Works, in the James river. The space occupied by the prisoners is about six acres, enclosed by an earthwork three feet in height; within this space were confined as many as 10,000 prisoners. The part occupied by the prisoners is a low, sandy, barren waste, exposed in summer to a burning sun, without the shadow of a single tree; and in winter to the damp and cold winds up the river; with a few miserable tents, in which perhaps one half the number were protected from the night fogs of a malarious region; the others lay upon the ground in the open air. One of them said to me: 'We lay in rows, like hogs, in winter, and take turns who has the outside row.' In the morning the row of the previous night was plainly marked by the bodies of those who were sleeping on in their last sleep. . . . Men were without medical treatment on the island until disease was so far advanced that when taken away in ambulances to the hospital, in squads of twenty, one half of them have died within five hours; some of them while their names were being taken at the hospital. . . . Men were returned from the hospital to the island when so weak that they have been obliged to crawl upon their hands and knees part of the way. . . . . *Libby* (prison) *mined.* Upon the approach of Kilpatrick, on his grand raid to Richmond, about the 1st March, the greatest consternation was produced amongst the inhabitants. The authorities felt sure of his ability to enter the city and free the prisoners. We were informed one morning by the negroes who labour round the prison that during the night they had been engaged in excavating a large hole under the centre of the building, and that a large quantity of powder had been placed therein. Major Turner said, in my presence, the day we were paroled, in answer to the question, 'Was the prison mined?' 'Yes,' and I would have blown you all to Hades before I would have suffered you to be rescued.' . . . . Bishop Johns said in the prison, when asked if he thought it was a Christian mode of warfare to blow up defenceless prisoners: 'He supposed the authorities were satisfied on that point, though he did not mean to justify it.'"

"Private Charles F. Pfounstich sworn and examined:—I am a German. Enlisted in 2nd Maryland, September 24, 1862; captured in Tennessee: imprisoned in Belle Island; reached there Jan. 21st; remained till 6th March. They took my blankets, sixty dollars in money, and a watch worth thirty dollars. For two days had no shelter; then I got in the tents; air came in on every side; two hundred men went in with me; the greater part had no tents; some had a blanket or old coat; some froze to death; could not keep warm; one of my regiment froze to death. Every morning we carried out some men froze to death, and from starvation some four or five men. We did not get enough to eat; ten or twelve ounces of corn bread and two spoons of beans, almost rotten. Sometimes we had soup, not fit to eat; yet we had to eat it. Had meat only three or four times while I was there; two or three ounces each time. I was hungry all the time. I could not sleep for hunger and cold, dirt and lice. I washed twice a day in James river. Strength kept up till last eight days; then I felt sick in my bowels. Had no diarrhœa. Did not go to the hospital. Left with the 9th Maryland. I saw a good many cases carried in a blanket to the doctor, and when they got there many of them were dead; had my feet frozen. There might be many deaths I did not see. I have reason to believe there was. I have stated what I saw, three or four a night. The men would dig holes in the ground to protect them from the air."

"Private Daniel M'Mahon sworn and examined:—Captured at Gettysburg; taken to Richmond; placed in Belle Isle; took my coat and blanket away; gave me no covering; a number of men had to lie out on the bare ground—two hundred. I was there till after Christmas. I suffered from cold very much, and so did the men more than I. We had cold rain storms. Some men froze to death in a ditch. It was not much better in the tents. I saw men carried out of the tents in blankets, dead. Saw this more than once. I suppose they died mostly from hunger and cold. We got about one third the loaf shown of corn bread (loaf weighed, weighs 15 ozs.) twice a day; sometimes but once; meat once regularly—a small piece about as big as my four fingers. Suffered from hunger at Belle Isle. Heard others complain. My guards were not hungry, for they would sometimes throw bread in to the prisoners. I have picked it up myself. It was better bread than ours; not so coarse. I saw a man kill a dog and eat part of it. He sold the rest of it. I got some."

These few depositions are descriptive only of a part of the sufferings and barbarous treatment to which the prisoners were subjected: no mention is made in them of what was authenticated by others, such as the shooting by the guard those in the Libby prison who were seen near a window; of the appropriation by the prison authorities, of the provisions, clothing, and comforts sent by the friends of the prisoners; of the disgusting condition of the floors and ground from accumulation of ordure and neglect of ordinary attention to cleanliness, &c. As before remarked, to have an adequate idea of the shocking state in which the prisoners were kept, it is necessary to read over all the details, and keep in mind the vast amount of disease produced, and its kinds, and the extraordinary mortality. And what renders these details more impressive and revolting, is the comparing them with the accounts given—and given on like sworn evidence, of the manner in which the Confederate prisoners were dealt with,—who, instead of being deprived of their clothing, had often better given them; instead of being kept on a famishing diet and treated worse than brute animals, were well fed and taken care of, so that when exchanged, for most part, they were in better health than when captured.

It was a question with the commission, whether the treatment the Federal prisoners were subjected to, was matter of necessity or of design,—of necessity from the straitened circumstances of the South, or of design, for the double purpose of intimidating the northern people from enlisting to repair the waste from war, and of increasing that waste by the vastly increased mortality, the result of the treatment. The conclusion arrived at by them was the latter; and, we must confess, that so far as we are able to judge from the evidence brought forward, it is difficult to come to any other. Even had the Confederates a scarcity of provisions, and their own men were underfed, of which there is proof to the contrary, that would have been no excuse for the worst part of the treatment—the allowing so many to be frozen to death, and the neglect of every approach to a regard for their health and comfort.

That the commission, in collecting evidence and drawing up their report, acted under a full and right sense of what was due to truth and justice we entertain no doubt. One little episode, which they make us acquainted with, may be mentioned in proof; one short passage describes it. Their words are—

> "It is a remarkably mortifying fact that some of our trials came from our own men. At Belle Isle and Andersonville there were among us a gang of desperate men ready to prey on their fellows. Not only thefts and robberies, but even murders were committed. Affairs became so serious at Camp Sumpter that an appeal was made to General Winter, who authorised an arrest and trial by a criminal court. Eighty-six were arrested; six were hung, besides others who were severely punished. These proceedings affected a marked change for the better."

This incident, shocking as it is, we may remark, is only confirmatory of the well-known fact of the demoralising influences of intense want and other degrading conditions of body and mind, and of which, as has often been stated in our pages, we have ample proof in our lower classes, when most distressed and neglected.

We should not omit to mention, that four photographs of Union soldiers after their return from imprisonment at Belle

Isle, are prefixed to the report, stated to be accurately copied from the originals, taken at the United States' General Hospital, Division No. 1, Annapolis, Maryland, and now in possession of the United States' Sanitary Commission; they are truly horrible, and most piteous!

ART. XIX.—*The Anthropological Treatises of Johann Friedrich Blumenbach, late Professor at Göttingen, &c. With Memoirs of him by Marx and Flourens, and an Account of his Anthropological Museum, by Professor M. Wagner, and the Inaugural Dissertation of John Hunter, M.D., on the Varieties of Man.* Translated and Edited from the Latin, German, and French originals, by THOMAS BENDYSHE, M.A., V.P.A.S.L., Fellow of King's College, Cambridge. *London,* 1865. Pp. 406.

THIS work, one of the publications of the Anthropological Society, is creditable to the Council of that body, inasmuch as the views taken of man by Blumenbach and his biographers concentrating in the unity of the human race, are not in accord with those of some of its members. The last paragraph of the 'Eloge,' by the perpetual secretary of the institute of which Blumenbach was one of its distinguished foreign members, is strong on this point, and is worth quoting also from the impression it conveys, in a few eloquent words, of the character and career of the individual.

> "Blumenbach died on the 22nd January, 1840, being nearly a century old;[1] a man of the highest intellect; an almost universal scholar, philosopher and sage; a naturalist, who had the glory, or rather the good fortune, of making natural history the means of proclaiming the noblest and, without doubt, the highest truth that natural history has ever proclaimed. The *physical unity*, and through the *physical unity* the *moral unity*, of the human race."

And Professor Marx remarks—

> "At the time when the negroes and the savages were still considered as half animals, and no one had conceived the idea of the emancipation of the slaves, Blumenbach raised his voice, and showed that their psychical qualities were not inferior to those of the European; and that, even amongst the latter themselves, the greatest possible differences existed—that opportunity alone was wanting for the development of their highest faculties."

The editor, Mr. Bendyshe, has, we think, acted judiciously in giving the two sketches of this admirable man; the one from the German point of view, more strictly biographical, the other from that of the French academician, less biographical than scientific and literary. Besides being editor, Mr. Bendyshe is the translator of the several portions which contitute the volume. This his task we cannot so well commend; it has no pretensions to elegance, and though always intelligible it is not always grammatical; suggestive of a want of careful revision.

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[1] He was born at Gotha on the 11th May, 1752.

Short as the two memoirs are, they convey a very vivid idea of the man in all his varied relations of an active and prolonged life, and are as interesting as they are instructing. Both of them have been written as if on the principle that of the dead nothing but their virtues should be proclaimed; and rising from the perusal, we feel that such was his worth that truly nothing could be advanced to his disadvantage. What specially distinguished him were his love of truth, his indefatigable industry, and his love of research. To these qualities and great natural abilities or capacity, and a generous nature, he owed his success, his popularity, and the high estimation in which he was held. The very few anecdotes which are introduced are of a telling kind, and very effective. We shall transcribe two or three which are most characteristic.

When, after the peace of Tilsit, he formed one of a deputation to solicit the protection of Napoleon for the town of Göttingen, the following is related of him—

"Admitted at last to take leave in solemn audience, he attended in an ante-chamber with many of the foreign ambassadors; Napoleon appeared, all turned their attention to him except Blumenbach; for how could he? 'I had,' said he, 'before me the ambassadors of Persia and Morocco—of two nations whom I had never seen before."

"Being entertained in London by all the English professors, they one evening took him to the theatre. The actor Kemble played the part of the 'Moor of Venice.' Some days after, Kemble met Blumenbach at a party, and said, 'M. Blumenbach, how do you think I succeeded in representing the character of a negro?' 'Well enough, as far as the moral character goes,' said our naturalist, and then added, 'But all the illusion was destroyed for me the moment you opened your hand, for you had on black gloves, and the negroes have the inside of the hand of a flesh-colour.' Every one laughed except Blumenbach; he had spoken quite in earnest."

"After more than forty years spent in education, he wrote these words: 'I never enter the amphitheatre without having particularly prepared each lesson, for I know that many professors have lost reputation by thinking they knew well enough a course they have delivered twenty times.' He worked up to the end of his life. He said, 'I only know satiety by reputation.'"

Professor Marx says—

"In his note book I find written down the following remark: 'In the delivery of my lectures, as in my writings, I have always endeavoured to follow Quintilian's pattern! This is it. I tried to throw some brilliancy, not for the sake of displaying my genius, but that in this way I might more readily attract youth to the acquaintance of those things which are considered necessary for study. For it seemed possible that if the lecture had any thing pleasant in it, they would be more glad to learn, whereas a dry and barren mode of teaching would probably turn their minds away, and grate rudely against ears tender by nature.'"

In a foot-note it is added: "Purists were a nuisance to him. To call granite *kornstein,* he said, made him shudder." Also, that—

"He always tried to correct the improper use of definite words, especially with a view to the language of natural history: viz., 'My canary bird sings beautifully.' 'To hear a canary bird *sing* I would go ten miles; but perhaps it *pipes*'—'Yes, pipes, sings.' 'Ah, ah, now we understand each other.'"

"If you ever got him to talk on the chapter of writing, he took care never to forget to recommend the art of writing handily in your pocket, [sic] which had been of great service to him on diplomatic missions, through the agency of a short thick lead pencil and strong parchment paper."

His indulgence as to the vanity of authors may be appreciated by his saying—

"If a toad could speak and were asked which was the loveliest creature on God's earth, it would say, simpering, that modesty forbade it to give a real opinion on that point."

His principal works, on which his European reputation was founded and to which he will owe his enduring fame, were four, as named by M. Flourens. The first, on the 'Human Species;' the second, on 'Natural History;' the third, on 'Physiology;' the fourth, on 'Comparative Anatomy.' "These," his biographer says, "give us pretty well the whole of his great course of instruction." As an original writer, his claims are scarcely of the highest kind, for he can hardly be said to have made any great discoveries.

"His glory" (says M. Flourens) "is that he preceded Cuvier," adding: "There was, indeed, between these two famous men more than one relation; both introduced comparative anatomy into their own country; both created a new science, the one anthropology, the other the science of fossil anatomy; both conceived the science of animal organization in its entirety; but G. Cuvier, impelled by a great bias towards abstract combinations, did more to display a method; whilst Blumenbach, guided by a most delicate sensibility, did more to elucidate physiology."

Further, M. Flourens remarks—

"He submitted the great question of the *formation of beings* to the most profound researches,[1] and always as a physiologist. Facts were his study, and from facts he tried to mount up to the force which produced them. Nothing is more famous than the formative force of M. Blumenbach—his nisus formativus, which he so justly says, 'is only a mode of expressing a fact, like *irritability*, or *sensibility*, and whatever may be said of it, it is not more obscure.' For, he adds, 'every *original force* is obscure, from the very reason that it is *original*.' 'The first veil' (says Fontenelle) 'which covered the Isis of the Egyptians has been lifted a long time, a second, if you please, has been so in our time; a third never will be, if it really is the last.'"

No words of ours are needed to recommend this volume to our readers, the merits of the dissertations of Blumenbach which it contains having been so long known and fully acknowledged; and though the same cannot be said of the Inaugural Dissertation of Dr. John Hunter, yet, we think, it only requires to be perused to be approved. Considering that it was the production of a very young man, it must be held to be one of no common ability, and this in point of information, logical reasoning, and philosophical spirit, especially taking into account the then novelty of the inquiry and the little advanced state of physiology. Hardly justice, hitherto, has been done to its author. He might have been better known had he not been eclipsed by the celebrated physiologist, of the same name, his contemporary and countryman. All his writings that we are acquainted with, and especially the only paper he communicated to the Royal Society,[2] are indicative of a mind worthy of the name it bore; and yet, strange to say, we have found no recognition of it in any biographical dictionary.

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[1] "And through them he made the beautiful discovery of the umbilical vesicle of the mammals."

[2] Its title was "Some Observations on the Heat of Rocks and Springs in the Island of Jamaica, and on the Temperature of the Earth below the Surface in Different Climates." It was published in the 'Philosophical Transactions' for 1788. Dr. Hunter served in Jamaica as physician to the forces.

ART. XX.—1. *Defectus Uteri et Vaginæ.* Ved Dr. F. C. FAYE, Professor i Accouchement ved Norges Universitet, Overlæge ved Födselsstiftelsen og Börnehospitalet i Christiania. 8vo. Pp. 17.

On Absence of the Uterus and Vagina. By Dr. F. C. FAYE, Professor of Midwifery in the University of Norway, First Physician to the Lying-in Institution and Hospital for Children in Christiania.

2. *Inflammatoriske, hypertrophiske og fibröse Svulsttilstande af Livmoderhalsen.* Ved Dr. F. C. FAYE. 8vo. Pp. 56.

On Inflammatory, Hypertrophic, and Fibrous Tumours of the Cervix Uteri. By Dr. F. C. FAYE.

THOUGH ancient medical literature contains scarcely any well-established case of total absence of the uterus, such instances have in modern times, in consequence, no doubt, of more careful observation, not unfrequently been met with. Among those on record we may distinguish between the cases where more or less rudiment of the organ has been found, and those in which there was total absence of the uterus. In some instances the ovaries and Fallopian tubes have been normally formed; in others the development of these organs also has been arrested. In a case observed by Bousquet, both uterus and urinary bladder were wanting. Morgagni and Dupuytren met with several cases of absence of the uterus and vagina. It was supposed that where the uterus was absent, the breasts would be but slightly developed and that there would be no sexual desire; but direct experience has proved the opposite with regard to the breasts, and there is reason to infer that in many cases sexual desire has also been present. Even menstrual molimina, depending chiefly on the activity of the ovaries, may occur.

The diagnosis of congenital absence of the uterus can scarcely be established with certainty in the living subject, unless the other signs on which it is based be confirmed by the results of physical examination. The latter is best made by introducing a catheter, not too short, and curved towards its free extremity, or else a knobbed sound, into the bladder, which should be rather full, while the index finger of one hand is passed as high as possible into the rectum. On now turning the curve of the catheter so that the point of the instrument may be directed backwards, the finger on the rectum will meet the sound, if no uterine body intervenes; and in this way the examination can be carried tolerably high up into the pelvis, the point of the sound being easily felt through the comparatively thin walls of the bladder and rectum.

After the foregoing, and some additional remarks, the author proceeds to relate a case which recently occurred to him. The patient, aged thirty, had always enjoyed good health until her twentieth year, when she began to suffer from determination of blood to the head, with languor and general indisposition. She laboured also under amenorrhœa, pain in the back, and some difficulty in passing water. It was not until after the lapse of ten years, in March, 1865, that she consulted Professor Faye, having previously been at different times under the care of five or six physicians. She had also, about the first year of her ill health, been betrothed. She was now, after a previous oral investigation, subjected, with the aid of Herr Schönberg, assistant physician to the Lying-in Institution, and of the head midwife, to a careful examination. The external genitals, with the exception of an only rudimentary clitoris, were found to be normally developed; and within the labia minora, which passed up tolerably high, without, as usual, forming two distinct crura about the clitoris, was seen a vestibulum also of the ordinary form. But with this the genital organs, so far as could be ascertained from without, terminated, not the least trace of any opening indicating an introitus vaginæ being discoverable. The fundus was, however, rather soft and yielding, so that by persistent pressure with the finger a funnel-shaped little sac could be formed internally in the pelvic opening. The abdomen presented nothing abnormal, nor was there any sign of distension; the breasts were well developed, the patient's figure and appearance were feminine, leading to the inference that the ovaries were present and probably active. No uterine body could be discovered on internal examination, but in the situation of the cervix uteri was felt a small uneven flat tumour, of the size of a bean, apparently formed of connective tissue, but perhaps rudimentary of the vaginal portion of the uterus. No trace of vagina could, on internal examination, be discovered. There was so little connective tissue between the bladder and the rectum that the finger received a very distinct impression of the sound.

The author proceeds to describe the uncertainty in which the parties principally interested had been kept for the space of ten years, in consequence of the true nature of the foregoing case not having been explained to them until they applied to him. Under the circumstances, it was evidently the duty of the physician to be quite explicit, though in another instance quoted by Professor Faye, where the parties had been already married, and where only the uterus was deficient, humanity might have suggested an opposite course. Professor Faye adds to his own observation brief notices of analogous cases already on record.

In the course of the second pamphlet quoted at the head of this notice, Professor Faye describes a case of unusually large fibrous tumour growing from the anterior lip of the vaginal portion of the uterus. Its length (heighth) was 5½ inches, its greatest diameter was 4½″, its greatest circumference was 12″. The author is not aware that such a tumour has been before found with its seat in this portion of the uterus. In general the first nucleus is formed higher up, and by its weight dilates the os uteri, forming, as the tumour descends, a stalk of greater or less thickness.

Art. XXI.—*Systems of Admissions at Hospitals.—A Plea for Reform: being a selection from a series of articles reprinted (by permission) from the 'Birmingham Daily Post.' With a Preface.—Birmingham,* 1864. Pp. 32.

This was a well-timed pamphlet, would that we could say no longer needed. The object of its author, as set forth by himself, is the admirable one of regulating the admission of hospital patients, so as to produce the maximum of good and the minimum of evil. This important and somewhat difficult subject is discussed in seven chapters following each other in logical sequence.

The author shows first, how and when the privileged system of admission began, tracing it back in spirit to the abolition of monasteries, and in action, especially in the provinces, to a comparatively recent period, the present century; the great majority of our county infirmaries and hospitals in our large towns having been founded within this period, and on the privileged "spurious charity plan."

He next treats of the evils of this system, with the object of showing, that whilst a large hospital in a great degree unendowed can exist and flourish without a privileged list of subscribers, a hospital depending on such subscribers must of necessity be more or less a failure. He remarks:

> "If no privileges were brought to bear on the admission of patients, the hospital would only admit those who really needed hospital assistance. The yearly admissions would be made to tally precisely with the yearly income. The managers could then say to their supporters, 'Give us your money in such proportion as you

deem fit. Let us have it unfettered. We will make it *our duty* to admit as large a number of proper persons annually as your gifts will permit. Beyond this we, like other charities, will not go. Should your contributions fail to meet the genuine demands of the hospital population, we shall from time to time make appeals to the general public, based upon incontrovertible facts.'"

In reply to a statement in opposition to the free system, viz., that a hospital cannot flourish if the subscribers possess no privileges, the author adduces instances of the contrary, examples of free hospitals with larger subscription-lists than the privileged hospitals can boast of; and he supports his arguments by the facts, that the noblest subscriptions, those of vastest amount and ever memorable, such as that for Ireland during its famine-distress, that for Lancashire during the cotton scarcity, have always been free, founded on the true principle of Christian charity, that the left hand should not know what the right hand doeth; always keeping in mind as a guiding rule, that subscribers are not the persons best qualified to select the cases for admission, which, to be well made, must be regulated by competent judges, the hospital officials themselves.

On the evils of the one system, the privileged, and the advantages of the other, the free, on which the author further amplifies, we trust we need not here dwell: our medical readers can fully understand how some of the former verge even on the absurd, such as the limit of time of hospital treatment irrespective of the state of the patients and the nature of the disease.

As a matter not of choice, but of necessity, the author treats of the best mode of working a privileged system, amounting, as we understand it, mainly to this, that the more the privileges of admission are curtailed, the more freedom is given to admission, the more the evils of this system will be checked. Of subordinate checks which he suggests, one is the issuing of tickets to subscribers twice instead of once a year; another, that before sending a patient, the subscriber should learn whether there is a vacancy in the house; a third, that to exercise his privilege a subscriber should apply for a ticket at the time it is wanted; thus rendering the matter of admission less trivial, and imposing more care in the selection of the objects. After describing these and some other checks, he remarks:

"At the best, however, every modification of the privileged plan leaves it with many stains. The free mode is alone compatible with financial success, an orderly method of administration, and the strict allocation of charitable funds to the persons for whom they were designed. It is the only one based on true charity, on the demands of humanity, or upon right reason. All other systems are in their inmost nature unsound and false. They are perishing, as all such things must perish. But so long as the poor suffer from physical disorders, and so long as richer men are influenced by generous impulses to aid their fellows in their necessities, so long will a plan which brings the needed relief into the closest, the quickest, and the surest operation, command the sympathies of the good and claim the judgments of the wise."

The author, before concluding, sketches briefly a model of a modified free method of admission, founded chiefly on this precaution, that in the tickets issued to subscribers no distinction be made between in and out-patient letters, the hospital authorities deciding according to the nature of the cases whether the bearers should be received into or treated out of the house; and further, that no persons residing at a distance should be received unless their cases be previously submitted for approval, and the certainty ascertained of a vacancy allowing of their admission.

Lastly, the author dwells on the precautions which the hospital managers are bound to take of the funds intrusted to them, keeping in view the welfare of the community, and the propriety of not encouraging a spirit of dependence and improvidence amongst the lower classes, and at the same time not neglecting the rights of the medical profession by the admission of well-to-do patients, at least those who can secure on easy terms medical aid by belonging to a club, that peculiar English institution; and he proposes a limitary scale of wages, varying in amount according as the individuals are single or married, with or without children, exclusive of cases of a special and exceptional kind needing inquiry, and proof of more than ordinary urgency. In his comprehensive view of hospital concerns the author, we are pleased to find, does not overlook those who, from the exigencies of their cases, though capable of paying a private practitioner, may be entitled to have the benefit of hospital treatment: such patients, we agree with him, should remunerate the establishment for the benefits they receive, and at a rate and under conditions determined by the executive.

We must not conclude our brief and imperfect analysis of this able pamphlet, written, as it evidently is from ample experience of hospital economies, and a thorough conviction of the advantages and disadvantages of the opposite systems, without giving its author, anonymous as he is, our tribute of praise, and recommending it strongly through our medical readers to the attention of governors of hospitals, and to the subscribers to these beneficent institutions; *i. e.*, when so managed as to do as much good as possible and as little evil.

ART. XXII.—1. *Chloroform; its Action and Administration.* A Handbook. By ARTHUR ERNEST SANSOM, M.D., London, &c. *London,* 1865. Pp. 192.

2. *On the safe Abolition of Pain in Labour and Surgical Operations by Anæsthesia with mixed Vapour.* By ROBERT ELLIS, Surgeon-Accoucheur, &c. *London,* 1866. Pp. 80.

IT is interesting to watch the progress of discovery; we find that those sciences which have attained the greatest eminence, and have proved of the greatest service to mankind, have had their origin either in some casual observation, such as that of galvanism on the contraction of the muscles of the frog when touched by the different metals; or from a happy inference, such as that of Sir Humphrey Davy, who from the effects of nitrous oxide when inhaled in arresting painful sensation offered the conjecture that this gas might be of use in surgical operations. But, and it is noteworthy, how differently the casual observation of the one and the suggestion of the other was received; whilst the former immediately excited attention and led to the rapid development of voltaic electricity and its collateral branches in quick succession, constituting a new domain in science; the latter remained dormant and neglected, and seems not to have been little thought of until after the death of its author, that is, after an interval of nearly half a century, when being subjected to the test of experiment, the idea was verified; and then, almost immediately, was followed by the discovery of other and more powerful anæsthetics, ether and chloroform, and with consequences in relation to medicine, especially surgery, of the most influential and beneficial kind, hardly ever yet fully appreciated, whether we consider them in their use for relieving human suffering or as aids in advancing operative art.

Of the two works before us on this great subject of anæsthetics, our notice must be brief. Dr. Sansom's, from its comprehensiveness—much more comprehensive than its title implies—hardly admits of analysis. Comprised in twenty chapters, it is exhaustive as to the manner of treatment. No part of the subject is neglected, not even its literature on chemistry; and most parts are discussed in a very satisfactory manner. It is no small recommendation that the author has himself engaged in the inquiry of anæsthetics experimentally, and has prepared himself for informing us by informing himself, after a large experience, in the use of these subtle agents. The chapters which, we think, will prove most interesting and useful, are those in which the action of chloroform and other anæsthetics is considered; the danger from their use; the danger of incautious administration; the signs of danger; the nature and mode of death from chloroform; resuscitation in apparent death; methods of administering chloroform; its uses in surgery, obstetric practice, in practical medicine and in dentistry. On all these important matters there is excellent information. The chapters in which they are contained are deserving not only of the careful perusal of the student, but of being consulted by the physiologist and the advanced practitioner.

That portion of the work relating to the physiology of anæsthetics, seems to us less satisfactory than those we have just commended, and may be deserving of revision, inasmuch as the hypothesis the author adopts is, that chloroform and all other anæsthetics act directly on the blood, disregarding any specific action from them on the several organs. That anæsthetic vapours when inhaled are absorbed by the blood, and act through its medium, cannot be doubted; but it seems to us almost as certain that they act also locally, and may have somewhat different effects on different organs, according to the analogy of those articles of the materia medica, the operation of which has been carefully studied. Did they take effect only in consequence of a toxic effect on the blood, ought they to have the effects they are known to produce when applied externally and partially?

The main object of Mr. Ellis's little work is, as its title implies, to show that anæsthesia may be accomplished with safety in labour and surgical operations by mixed vapour—those of ether, alcohol, and chloroform—administered by means of an apparatus, a very ingenious one of his own invention, of which figures are given, and which can hardly be described without the use of figures. By this his method he is of opinion the risk, small as it is from the employment of chloroform alone may be avoided, the alcoholic portion of the mixture acting as a general stimulant, the ether as a stimulant to the heart, both tending to counteract the depressing influence of chloroform on this organ (whence, as is generally admitted, the greatest danger), without interfering with its more powerful anæsthetic quality.

The great recommendation insisted on by the author, and, we think, rationally so, from the use of his process is, that, besides safety, there is a security from any disagreeable effects; it is ensured by the perfect manner in which the proportion of each vapour can be used so as to produce the influence desired; and, another recommendation which it certainly deserves is the comparatively small quantity of chloroform that is needed. The objects which the author has aimed at, and which he thinks he has attained by the method he advocates, are the following; in justice to him we shall state them in his own words:—

> "First. Entire security against the excessive action of either of the anæsthetics.
>
> "Secondly. The production of a modified anæsthetic, varying, at the will of the operator, from a mere feeling of exhiliration to the deep unconsciousness requisite for abolishing acute pain.
>
> "Thirdly. The power of maintaining this state of anæsthesia at the same degree for any requisite period, or for medifying it to arising exigencies.
>
> "Fourthly. The reduction of the dose of chloroform to its lowest practicable point.
>
> "Fifthly. The partial substitution for it of a vaporous basis of mixed alcohol and ether, whereby its properties are enhanced and sustained, and its dose diminished without abatement of its value as an anæsthetic.
>
> "Sixthly. The contraction of the heart-depressing power of chloroform, by combining it with a heart-stimulant, and thus obviating some of the most frequent causes of danger from choloformization."

We hope that a method so simple as this of Mr. Ellis, a modification and an improvement of older methods, of which an account will be found in Dr. Sansom's pages; the chapter "On Anæsthetic Mixtures" will have the trial it seems to deserve; and should it answer in other hands as well as in his, great indeed will be the benefit which it will confer!

ART. XXIII.—*Contributions to Dermatology.* By DR. McCALL ANDERSON. *On the Non-identity of the Parasites met with in Favus, Tinea Tonsurans, and Pityriasis versicolor.* Glasgow, 1866.

THE author, who is well known as an authority in dermatology, holds that the *Tricophyton,* the *Achorion Schonleinii,* and the *Microsporon furfur* are not identical, but distinct fungous growths; and the proofs of this proposition, he maintains, are to be found—1st, in the results of inoculation—each parasite producing its own disease—never one of the other; 2nd, clinically, each disease producing by contagion its own kind; 3rd, in the microscopic characters of the fungi; and 4th, in the occurrence of vegetable parasitic skin diseases amongst the lower animals, and their transmission to the human subject—

favus always giving rise to favus, and tinea tonsurans to tinea tonsurans. Interesting cases illustrative of the communication of these affections from animals to man, are detailed. The paper is exhaustive upon the subject of which it treats, and sustains the character of the author as a thinker and writer on skin diseases.

ART. XXIV.—*Gout and Rheumatism in relation to Diseases of the Heart.* By A. W. BARCLAY, M.D., Physician to St. George's Hospital, &c.—*London*: Churchill and Sons, 1866, pp. 214.

THE author of this work has been for many years a diligent and successful observer in the field of cardiac pathology. Since the publication of a valuable paper on the statistics of valvular lesions, and their recognised causes in the thirty-first volume of the 'Medico-Chirurgical Transactions' in the year 1848, he has been known as an original and careful investigator in this one of the most difficult as it is one of the most important of the departments of clinical study. His present work has nothing of the character of a systematic treatise. The author starts from the standpoint of the most commonly recognised causes of cardiac disease; he investigates those causes, comparing them with each other, and with allied but different pathological conditions; and supports views, which are frequently original, and differ from much of the common teaching of the day, by argument and the results of therapeutical experience. Whilst valuable lessons are taught in the diagnosis and pathology of heart disease, they are so introduced as to form a subordinate although by no means an unimportant part of his essay.

The first chapter is on the origin of gout, and in it Dr. Barclay breaks a lance with the chemical theory which, since the publication of Dr. Garrod's researches, has had an almost unchallenged sway. It is true that Dr. Barclay claims Dr. Gairdner as holding similar views with himself, but there can be no doubt that in almost every medical class room in Great Britain students are taught that gout is essentially the retention of uric acid in the system, and that if the uric acid can be got rid of the disease will be cured. Of course the presence of an excess of uric acid in the blood during the gouty paroxysm, and the deposition of urate of soda in the inflamed joints, are unquestioned, but Dr. Barclay argues that it is far too mechanical a view to hold that the former constitutes the essence of the disease gout, and that gouty inflammation is simply the effect of the latter. He asks whether, if inflammation of the bronchi or stomach occur during an attack of gout, and no urate of soda is to be found in their respective tissues, the bronchitis and gastritis are to be excluded from our idea of the disease. To him it appears that the fact that urate of soda is not found deposited in the bronchi when bronchitis has been a concomitant of gout, is of itself a proof that true gouty inflammation is not always associated with or caused by such deposit. Without insisting on Dr. Garrod's view "that true gouty inflammation is *always* accompanied with a deposition of urate of soda in the inflamed part," we think that it may be fairly pleaded that gouty bronchitis may have a purely chemical origin. The presence of an excess of uric acid in the blood, which circulates in such large quantities through the bronchial capillaries, may surely act as a local irritant, and explain the proclivity to bronchial inflammation. The same explanation will apply to the stomach, if, indeed, there be such a thing as gout in that organ, which some gastric pathologists, amongst whom is Dr. Brinton, hold to be a matter of very great doubt. The hereditary nature of gout, the fact that in health the kidneys readily excrete any excess of uric acid which may be produced, whilst in gout without any appreciable disease of their structure they refuse their office as far as this particular product is concerned, and the impotency of alkalies in comparison with specifics, such as colchicum, to arrest the disease, are other arguments wielded by Dr. Barclay against the purely chemical theory of its nature. It is to an alteration in the blood-globules that Dr. Barclay refers the first step in the production of the gouty diathesis. He holds this view in common with Dr. Gairdner. Of course, as he says, the means of confirming or contradicting such an hypothesis are as yet wanting, but he argues for its probability with much ingenuity.

"In our view of the subject the retention of the uric acid is a symptom, a consequence of the attack of gout, and not its cause. The good living and the stimulants do not simply cause an excess of uric acid to be formed; but they end by causing some more permanent change, and one probably affecting the blood-globules, which reacts on the kidney, putting a stop to the excretion of uric acid, and causing its retention in the serum, where, passing in the round of the circulation, it is very apt to become deposited as urate of soda. Experience would seem to lend its sanction to the latter view. No amount of artificial alkalescence of the blood and urine will put a stop to the paroxysm when it has once commenced. In old standing cases the subacute inflammatory condition will last for months, in spite of alkaline treatment fully and fairly carried out. No one who has compared in an early and acute attack the power of alkaline remedies with that of the so-called specifics, such as colchicum, in arresting its progress, can doubt that there is a disease to which the name of gout is applied, distinct from the excess of uric acid in the blood-serum which attends its progress."—P. 23.

Of course, for this argument to possess validity, it must be clearly proved that colchicum, and other so-called specifics, do not give relief by virtue of their acting on the excreting organs. If, under colchicum, the kidneys or bowels are stimulated to excrete uric acid, and this process is coincident with arrest of the gouty paroxysm, the argument from therapeutics is, at least, greatly weakened. The impotency of alkalies may, after all, be a question of degree. Most practical men, when they give colchicum, combine alkalies with it; and we think that experience has amply proved that potash and lithia, if they do not cure gout, are very valuable adjuncts to a specific treatment. Again, we can hardly conceive that such a medicine as colchicum can exert an influence on the blood, or on its corpuscles, except by aiding an eliminative process.

The Second Chapter is on Acute Rheumatism in relation to Heart Disease. The complete separation of rheumatism from gout, the difference of each from the so-called rheumatic gout—the osteo-arthritis of Dr. Adams,—the specific differentiation between acute rheumatism and the muscular and chronic, the suppurative and synovial forms of the disease, acquire the highest practical importance from the fact that it is almost alone to acute rheumatism that disease of the heart of rheumatic origin can be ascribed. In adult age, in subacute rheumatism, *i. e.* acute rheumatism deficient in inflammatory action and febrile disturbance, Dr. Barclay believes that the risk of cardiac inflammation is very small. A certain amount of inflammation and fever he holds to be essential to its development. In childhood, however, any rheumatic attack with febrile symptoms, although the joint affection be slight, is likely to give rise to cardiac mischief. In after life the chances will be in proportion to the amount of local inflammatory action and general fever, although they are greatly diminished after adult age is complete. This chapter contains some valuable practical distinctions between the different forms of rheumatic disease; but we must pass on to the next, which treats of the symptoms and progress of Rheumatic Inflammation of the Heart.

Dr. Barclay believes that the first auscultatory sign of the supervention of cardiac inflammation which is to be distinguished in acute rheumatism is an alteration in the rhythm of the heart's sounds. The first sound is no longer to be distinguished in length from the second. It is not that the diastolic sound becomes prolonged, but the systole is shortened, and the change is explicable by the effect of inflammation on muscular contraction. Accordingly, it is more evident in pericarditis than endocarditis. On the difficult subject of the diagnosis of exocardial friction from endo-cardial murmur, the author has some excellent practical observations. He ascribes only a degree of minor importance to the character of the sound, its relative degree of apparent nearness, and even to an extension of cardiac dulness and absence of respiratory murmur over the cardiac region, as grounds for pronouncing in favour of either. He insists especially on the difference which we believe was first pointed out by Skoda, viz., that friction sound differs from endocardial murmur in its relation to the time of occurrence and duration of the ordinary first and second sounds. In the large majority of cases, friction sound is double; but there is no semblance of first and second sounds, and it is impossible, as far as the to-and-fro sound is concerned, to say which is systole and which diastole of the heart. It does not correspond with the rhythm of the heart's action; and the ear soon appreciates the difference if the systole and diastole be listened to at a little distance along the aorta, for instance, and then it be applied over the point of the abnormal sound. Another point on which Dr. Barclay's remarks deserve careful study, is that of the differentiation of functional from organic murmurs. We have not space to reproduce his observations at any length; but the following are the principal points on which he insists. The systolic aortic murmur is that most likely to be similated by a functional one. The character of the two murmurs affords no information; but it has been said that a functional murmur may be traced most distinctly upwards, towards the left shoulder, while a valvular murmur is heard more clearly across the sternum. Dr. Barclay shows the truth and error of this proposition. It is quite true that a murmur heard over the second intercostal space on the left side is generally functional, because organic disease at the pulmonic orifice is excessively rare, and a murmur in the pulmonary artery is almost always functional in its origin. But it is not true that a murmur heard across the sternum, at the second right intercostal space, is always, or even generally, organic. Aortic murmur may be either functional or organic. If organic, it will have its point of greatest intensity clearly marked, just after the blood has passed the seat of disease; whereas a functional murmur, depending on change in the constitution of the blood, is a diffused murmur. Dr. Barclay's experience leads him to say very positively that when a murmur can be traced some distance along the aorta, there is always present a changed condition of the blood.

> "The vibration is by this means propagated along the artery equally when it takes its rise in some obstruction at the aortic orifice, and when it is due to the passage of unhealthy blood from a large cavity into a narrow tube, as is the case in simple functional murmurs. I am quite certain that the very roughest and loudest murmurs are sometimes scarcely audible on the right side of the sternum, and that the most conclusively functional murmurs are often louder there than at the base of the heart."—P. 62.

The two great indications for diagnosis are, first, the power of producing murmur in the great veins and carotid artery by the pressure of the stethoscope in the case of functional murmur; and, secondly, the greater intensity of organic murmur near the spot where it is produced. It is not necessary that a blood-murmur should originate at a valvular orifice. If the aptitude of the blood for being thrown into vibration be great, blood-murmur may be produced in the cavity of the ventricle itself, probably excited there by the projecting muscular bands of its inner surface. This Dr. Barclay believes to be the explanation of a fact which presented itself to his observation long ago—that a murmur at the apex does not always imply mitral regurgitation. Such a murmur would be distinguished from a mitral murmur by the absence of a definite point of greater intensity. Still, although not defined, there is generally a limited area over which a functional murmur is heard louder than elsewhere; and the author's experience has led him to recognise three localising circumstances: first, the tendency to be produced at the commencement of a tube, whether aorta or pulmonary artery; second, the sound being loudest when the lung does not interpose between the blood and the ear; and thirdly, it being more readily transmitted through the intercostal space than through the rib.

Very different from these murmurs is the mitral regurgitant if it be heard before hypertrophy and dilatation of the heart have set in. It is then that the true *bruit de soufflet* heard definite distinct and sharp, generally on a level with the apex beat, and about an inch nearer the sternum contrasts most strongly with the diffused character of the blood murmur. This distinctness is lost when hypertrophy and dilatation supervene, and especially if there be accompanying irregularity of the heart's action. An important observation, which, however, is well known to the profession, is, that in acute rheumatism the blood may be so altered as to produce functional murmur.

We have dwelt at such length on these difficult although most essential points of diagnosis, that we have left ourselves no space to follow our author in his observations on the treatment of rheumatic fever and inflammation of the heart, on the progress of the latter, and in his reasonings on the connection of gout with degenerative cardiac disease and atheroma. This is perhaps less to be regretted, as we leave our readers the inducement to consult for themselves a little work which cannot fail to afford instruction and material for thought. The results of Dr. Barclay's actual bedside observation as given in this volume are most valuable, and he is never in want of forcible arguments and practical illustrations to support his pathological views.

PART THIRD.

Original Communications.

ART. I.

Essay on the Medicine of the Greeks. By T. CLIFFORD ALLBUTT, B.A., M.B. Cantab., Physician to the Leeds Infirmary, &c. &c.

Εἶσι δὲ οὗτοι οἱ οὐδὲν ἄλλο οἰόμενοι εἶναι ἣ οὗ ἂν δύνωνται ἀπρὶξ τοῖν χεροῖν λαβεσϑαι, πράξεις τε καὶ γενέσεις καὶ πᾶν τὸ ἀόρατον οὐκ ἀποδεχόμενοι ὥς ἐν οὐσίας μέρει.

Καὶ μὲν δὴ, Ὦ Σώκρατες, σκληρούς γε λέγεις καὶ ἀντιτύπους ἀνθρώπους. *Theætetus*, p. 155. Ed. Steph.

(*Continued from* vol. xxxvi, p. 185.)

THOSE who study history calmly and thoughtfully, who welcome all kinds of knowledge, and whose minds are sensitive to impressions from every side, must often lose themselves in wonder at the complexity of earthly things. Most people read history along certain lines or within circles described by their own prepossessions or by the accident of teaching; yet even for them has the complexity of human life become a matter of commonplace assertion, and at times, perhaps, of enlightened perception. How strange that the most exquisite civilisation the world has seen should have so perished that the memorials of it are matters of especial research—that a tradition which should animate whole nations is now only food for a few scholars and a very few artists!

The flow of Greek life lasted but a brief while; and if its sweet waters have reached us, like many other waters, they have flowed so long underground that their source is forgotten.

About the time of Pericles this brilliant people shone like a foam on the darker world, and as quickly vanished; many of its records were lost for centuries, and are now but partially recovered. What is sadder than all, the Greek civilisation was not only broken up and its pure light sullied by imperial and mediæval obscurity, but wayward nature has turned that into a scandal which is in its own place full of lively uses and proper beauty.

When the spirit of Hippocrates, Plato, and Aristotle, was dead, men clothed themselves with the letter, as French women used to masquerade in Greek costumes, and as modern architects build mediæval churches. The light may be a curtain as well as the darkness, and thus the authority of genuine thinkers and observers became a bondage and their names as the names of idols.

"*M. Tomés.*—Cela est impossible. Hippocrate dit que ces sortes de maladies ne se terminent qu'au quatorze ou au vingt-un, et il n'y a que six jours qu'il est tombé malade.

"*Lisette.*—Hippocrate dira ce qu'il lui plaira, mais le cocher est mort."[1]

Thus the bones of the King of Edom are burned for lime.

Later philosophers and later artists have preferred parts to the whole. They have elaborated infinite webs of subtilty and infinite fantasies of decoration, and they have forgotten simplicity of conception and distinction of purpose. To spin conclusions upon the authority of Hippocrates or Aristotle, is to show utter ignorance of the conditions of knowledge.[2] The Greeks had, no doubt, the conditions necessary to certain kinds of artistic perfection. They had wealth of legend, sufficiency of imagination, and intellectual power. Hence in poetry, which beside these needed only actor and audience, and in sculpture, which beside them needed only pure marble and keen tools, they were able to place themselves above the acquirements of the past, and in sculpture above the hopes of the future. Pictorial art, on the other hand, requires means of representation, which even Polygnotus possessed very partially. So supremacy in painting was closed to the Greeks inexorably. On foundations still more deficient were they obliged to erect their schemes of polity and of medicine. The accumulation of facts is a work of generations of labour, and of multitudes of workers co-operating to that end, while their verification depends on a multiplicity of opportunities and on an accuracy of instrumental agency which for the Greeks was impossible.[3] Plato and Aristotle wrote upon politics without the help of historical comparison, and Hippocrates upon medicine with the help only of a few observations, and those ill-verified. Our means, therefore, surpass theirs, more than their keen and great intellects surpass our own.

Happily, Hippocrates was, perhaps by happiness of birth, entirely free of that mental habit which, with few exceptions, has palsied more or less all useful thought in later ages. That which gave me the intense pleasure I found on first reading Hippocrates, was not his doctrine as it stood alone, but rather the curiously delicate poise of his mind. Our minds, when compared with his, are too often like loaded dice, which must cast up certain opinions. We begin to long for freer play, even if to turn up lower numbers. Since the time of the Greeks men's minds have become enslaved to authoritative principles, and the vice of all thought in the medical, as well as in the philosophical and theological schools, has been a blind adhesion to certain assumed principles, and the refusal of facts which will not be tortured into conformity with them. Even now, many able physicians cannot help taking off their hats to venerable predispositions concerning the essential nature of diseases and the like, and to certain most respectable words, such as our old friend "Inflammation." So that, as Bacon says, there is too often among us "sequacitas potius et coitio quam consensus." The Greeks had a proper horror of thus building their minds to receive certain furniture. As Plato says in the seventh book of the 'Republic,' "though people may dream about real existence, they cannot behold it in a waking state so long as they use hypotheses which they leave unexamined, and of which they can give no account. For when a person assumes a first principle which he does not know, on which unknown first principle depends the web of intermediate propositions and the final conclusion—by what possibility can such mere admissions ever constitute science? It is indeed impossible."[1]

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[1] 'L'Amour Medécin,' Act ii, sc. 2.

[2] Aristotle himself says, 'De Rep.,' iii, 16—"ὅτι τὸ κατὰ γράμματα ἰατρεύεσθαι φᾶυλον."

Cf. also Euripides (Frag. ap. Stob.):

Πρὸς τὴν νόσον τοι καὶ τὸν ἰατρὸν χρεὼν
'ιδόντ' ἀκεῖσθαι, μὴ 'πιτακτὰ φάρμακα
διδόντ' ἐὰν μὴ ταῦτα τῇ νόσῳ πρέπῃ.

[3] All this is too admirably put by Mr. Lewes in his treatise on Aristotle for me to repeat it.

While, then, we may see falsity of fact and erroneous reason therewith in every treatise of Hippocrates, let me remind the reader that to study Hippocrates for information and as a co-ordinating authority on matter of fact is to use an old bottle for new wine; but to study him as I would have the reader do in admiration of his delicacy of observation, of his single love of truth, of his high and honorable temper, his great intellect, and withal his unwearied industry and devotion to his art, is not only to behold an example which few will forget, but it is also to give his proper fame to one of the greatest men of the greatest time of the world.

Physicians were held in peculiar honour by the Greeks, and I have mentioned the following sources of their doctrine and practice:[2]—the temples, whose uncertain and, probably, mischievous influence then, as always, found many votaries among the superstitious;[3] the gymnasiums, which carried training and diet to a high pitch of elaboration, and whose influence was undoubtedly much in aid of physiological medicine; finally, and chiefly, the great schools of medicine, such as those of Cos, Rhodes, Cnidus, Cyrene, and Magna Græcia. Permission from the state was required to give the right to practise, and the applicant was obliged to show that he had served a proper pupilage.[1] Moreover, as I have said, the speculations of the Ionic and Pythagorean schools played curiously upon medical doctrine, Hippocrates being one of the first to clear the art of medicine from such injurious influences and to place it as far as possible upon the sounder basis of induction.[2] The extant treatises on Greek medicine have come down to us in one collection under the name of Hippocrates. These have, however, been separated by modern critics, and so classified as to show that a minority only are from the hand of the great master.[3] Many more were probably written by his immediate followers, though some few do not come from the school of Cos, but from that of Cnidus.

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[1] Translation by Davies and Vaughan, Cambridge, 1866.

[2] See the 'Review' for January, 1866.

[3] Comp. Pausanias,' 2, 27, ii and x, 32, 8, et vid. Aristoph. Plut., 410, 653.

It is, perhaps, to be regretted that later editors of the Greek medical treatises have preserved the misleading title of the 'Works of Hippocrates.'

The list of them, roughly classified, is as follows:[4]

Class I.—'On Ancient Medicine;' 'On Regimen in Acute Diseases;' 'On Winds, Waters, and Localities;' 'On Epidemics,' books i and iii; 'On Articulations;' 'On Fractures;' 'The Surgery;' 'On Wounds of the Head;' 'On Instruments of Reduction.'

Of the treatises in this class we can give two, and two only, to Hippocrates on contemporary testimony, namely, the treatise on Fractures and that on Articulations. To these 'The Surgery' is, perhaps, a preface. It is, however, in a high degree probable that the remaining treatises in this class came also more or less completely from his hand.

Class II.—'The Prognostics;' 'The Coan Prenotions (now shown by Daremberg to be taken from the former, as against the converse opinion of Littré and Ermerins); 'The Prorrhetics,' book i (a disconnected mass of clinical notes). 'On The Sacred Disease;' 'The Doctor;' 'On the Use of Liquids;' 'On Wounds and on

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[1] It is clear also that some eminent physicians were in the pay of the state. Cf. Plato Gorgias, 455, also many places in Aristophanes, Strabo, &c.

[2] In the preference of reasoned truth and in the severance of it from cosmic speculations, Hippocrates much resembled Socrates.

[3] Some treatises are in actual opposition. Dr. Daremberg shows that this is the case with the 'Internal Affections, and the Aphorisms.' The second book of the 'Prorrhetics' also contradicts the 'Regimen in Acute Diseases,' and the first book of 'The Maladies' restricts the theories in the 'Critical Days.'

[4] I have not space to enter into the subtilties which encompass this classification. The reader will, however, find the matter more fully handled in the excellent article on Hippocrates by Dr. Greenhill, in 'Smith's Dictionary of Biography.'

Ulcers;' 'On Fistulas;' 'On Hæmorrhoids;' 'On Airs;' 'On Art;' 'On the Regions of Man;' 'On Dentition;' 'On Maladies,' book i; 'On Affections;' three books 'On Regimen,' with the treatise 'On Dreams;' 'The Epidemics,' books ii, iv, v, vi, vii; 'On the Humours;' 'The Oath.'

All these treatises belong to the Hippocratic school, the members of which, no doubt, copied much from one another. Time and the absence of early critical distinctions and testimony have made their classification a matter of much difficulty.

CLASS III.—'On Internal Affections;' 'On Maladies,' books ii and iii; 'On Regimen in Health;' 'On the Glands.' These treatises are referred on probable conjecture to the Cnidean school. That some Cnidian treatises were included in the collection from early times is ascertained by a reference of Erotian to the second book of the 'Maladies.' They may have formed part of the library of Hippocrates, and so became confused with his own writings.

CLASS IV consists of a series of early works on diseases of women, &c., written certainly not by Hippocrates, but probably by one person or in one school.—'On Diseases of Women,' books i and ii; 'On Barren Women;' 'On Diseases of Girls;' 'On Superfœtation;' 'On Excision of the Fœtus;' 'On the Seven Months' Child;' 'On the Eight Months' Child;' 'On Generation;' 'On the Nature of the Child;' 'The Maladies,' book iv.

CLASS V.—'On the Heart;' 'On Fleshes;' 'On Weeks.' These treatises M. Littré and some others suppose to be of the Aristotelian period. The supposition is, however, very uncertain.

CLASS VI.—'On Decorum;' 'Precepts;' 'On Dissection;' 'On Vision;' 'On the Nature of the Bones;' 'On Crises;' 'On Critical Days;' 'On Purgatives.' This class is a makeshift. It contains treatises of obscure origin, some of which, as says Dr. Daremberg on the 'Precepts,' deserve further examination.

Of Hippocrates, the author of the treatises in the first class, the reader will be surprised to find how little we know. Of his life we are as strangely ignorant as we are of the lives of the Greek dramatists, of Plato, and of other great writers of that period. There are, however, a multitude of tales concerning him, and these may be divided into three orders—1st, those which are true; 2nd, those which may be true; 3rd, and most abundantly, those which are false.

Now, as the facts of the second order have no more evidence upon which to rest than have those of the third, we are reduced, as Mr. Slime would say, to the ridiculously small sum of facts contained in the first category.[1] Omitting all legendary and doubtful matter, we learn that Hippocrates was born about 460 B.C., and that he lived contemporary with Democritus and Socrates, during a time of great mental activity in the departments both of philosophy and of medicine. He belonged to a medical family of the school of Cos, being himself the second of his name, and at that school he received the first instruction in his art. Though reputed to be of Asclepian family, there is little evidence to prove the supposition of Littré and others that he was in any direct way concerned with priestly functions. He travelled from place to place, practising and teaching medicine, and seems, with others, to have established a kind of doctrine, which he strongly urged against the opposing doctrines of the rival school of Cnidus. He received great honour and reward, and his name became, even during his lifetime, of proverbial renown.[2] He is not mentioned by Lucian in his list of old men,[3] but seems to have outlived the Peloponnesian war. After his death his fame waxed more and more, until the weight of it repressed all freedom of thought, and gave rise to a blind idolatry and a miraculous legend, which have lasted twenty centuries. Even yet, as Dr. Daremberg wittily says,—"On exalte beaucoup Hippocrate; mais on ne lit guère, et pour n'avoir rien à se reprocher, on sacrifie pieusement à un dieu inconnu."[4]

The treatises attributed to Hippocrates were collected at a very early period, probably before the time of Aristotle, who seems to some to have read them. Little or nothing has since been added. Subsequently they were obtained by the Ptolemies; and the Alexandrian commentators, though they had misgivings about certain books, yet seem to have been obliged to accept the canon pretty much as they received it. Such is the collection we now have; M. Littré thinks that they were unpublished at the time of Aristotle. The reader will agree with me in wondering how the great reputation of Hippocrates was obtained if his writings were unknown. They may, however, have been chiefly confined to a large body of scholars, as were the works of Plato; and the able remarks of Mr. Grote[5] upon these latter will be read with much interest by those who are studying the history of the Hippocratic collection. The collection has served as the groundwork for immense fabrics of commentary and lexicography. The once celebrated 'Lexica Hippocratea' have nearly all perished. Callimachus, the physician, whose commentary on 'The Articulations' is still extant, wrote one. The lexicon of Erotian happily remains to us, and is most valuable for its references to 'Scripta deperdita.'[1] The lexicon of Galen is the only waif which has reached us of his purely literary productions. These two lexicons were well edited at Leipsic, in 1780,[2] and may be used with the indispensable 'Œconomia' of Foesius. If the reader delights in the elusive delicacies of verbal expression, he will value these two books very highly. With the exception mentioned above, the vast number of commentaries written upon the collection before the time of Galen have fallen among the rats, a blessing whose magnitude it is hard to appreciate.

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[1] It is a matter of regret that Dr. Adams, the learned translator of the works of Hippocrates for the old Sydenham Society, should have shown so little critical power in dealing with the materials of the biography.

[2] Cf. Plato, 'Phædrus,' p. 470, Protagoras, p. 311, and other passages.

[3] 'De Longævis.' This observation I owe to M. Houdart, 'Études,' p. 69.

[4] 'Œuvres choisies d'Hippocrate,' Paris, 1855, p. xlvii.

[5] 'Plato,' by George Grote, F.R.S. London, 1865, vol. i. chaps. iv and in Compare his remarks with those of M. Littré on the preservation of books v. ancient times.

The learned, elegant, and profuse commentaries of Galen had a reputation which has preserved the medical portion of them to this day, and has probably caused the neglect and loss of all others. To them we chiefly owe the consolidation of the empire of Hippocrates, and the seal of that authority which has been turned by mediæval perversity rather to a curse than a blessing. My space obliges me to omit mention of the various editions of the collection and the efforts of critics therewith; they are of no real importance when compared with that by one of the most learned of modern scholars, to which I have often referred. The name of M. Littré will henceforth have the proud honour of inseparable association with that of Hippocrates; and it is, I think, Dr. Daremberg who pays him the elegant and well-deserved compliment of comparing his influence upon the literature of Greek medicine to that of the Renaissance upon the poets and orators. A short description of M. Littré's edition I gave in the 'Review' of January last, and it is the edition to which my references will be made.[3]

As I can only pretend here to give a general view of Greek medicine, I must not trouble the reader with any questions of minute scholarship, interesting and curious as many of them are, but must pass at once to consider the principal extant treatises, confining myself mostly to the authentic works of Hippocrates. Of these by far the finest is the magnificent treatise 'On Winds, Waters, and Localities.' I shall speak of this first, partly because in vigour of treatment and in greatness of conception it represents to us the finest characters of Greek medical writing, partly because it best shows the transcendent mental powers of its author. This treatise seems to be a generalisation made from observations partly recorded in the first and third books of 'The Epidemics,' where we see Hippocrates observing in detail the seasonal variations of disease.

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[1] Dr. Daremberg discovers that we possess some remains also of the lexicon of Bacchius (vid. 'Notices et Extraits des Manuscrits,' part i).

[2] That of Erotian is included also by Foesius in the Genevan edition of 1657.

[3] A splendid edition of the Greek medical treatises has been published by Ermerins in three thick vols. 4to, Utrecht, 1859, 1862, and 1864. I do not possess the book, and somewhat distrust it, from what I know of Ermerins' boldness in manipulating texts. Dr. Greenhill, however, tells me that he has a high opinion of its general excellence, but adds that it is difficult really to know what it contains, as, like the Aretæus of the same editor, it is not clearly arranged, and is deficient in indexes, &c.

I shall give an analysis of the whole treatise, one necessarily brief, but I hope not too brief for the good understanding of the great merit of the work. Hippocrates thus enters upon his subject:—"He who would rightly investigate medical science must do as follows. He must study the seasons, which not only differ from each other, but present also many variations within themselves. He must study the winds also, the cold and the warm; both those which are common to all countries and those which are found in particular districts. He must likewise look to the quality of the waters—whether they come from pools or springs, and whether they be soft or saline and harsh. On entering any city, too, he will notice the lie of the land to the wind, the sun, and the points of the compass; to the character of the soil also—whether it be bare, dry, and cold, or moist, wooded, and hot. Lastly, he will look at the life the people lead—whether they be lazy and gluttonous, or active, enterprising, and temperate. The physician who has made such observations will, on coming to a fresh place, have some readiness in dealing with the local diseases, and will make no serious mistakes. He will be able also to foretell the seasonal diseases, and the kind of results which follow the breaking of the laws of health. For whatever some people may say, meteorology and medicine are closely connected, the state of the human viscera changing with the seasons."[1] Hippocrates now proceeds to take these subjects severally; and first of all he says of *Aspects*:—"To look more clearly. Suppose a city to be exposed to warm *Southerly* winds. The people will be of a catarrhal habit, flabby, and of lax tissues, sluggish appetite and loose bowels. Their lives also will be short. The women, though not naturally barren, will suffer from leucorrhœa and be liable to abortion. The young children will have convulsions. The men will suffer from remittent fever, dysentery, fluxes, and piles. There will be few acute fevers or inflammations, and wounds will tend to take on an unhealthy character. Widespread diseases, however, will, of course, affect them as others.

"In a town which is exposed to cold winds from the *North*[2] the men are wiry, of bilious temperament, and little liable to fluxes or catarrhs. They will suffer rather from diseases of the upper viscera, such as pleurisies, and similar acute affections. They eat much, and so drink sparingly, as men do not do both in excess. Their ophthalmias are more acute and destructive than among the former people. Their lives are longer, and their wounds heal well. Their manners are somewhat rough. Puberty among the women is late, and they do not menstruate freely; hence they are often barren or liable to hard labours, but not to abortion. Their milk is also scanty; after childbirth phthisis oftens shows itself, their tenser internal fibres being more susceptible of rupture from strain.[1]

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[1] Cf. the following passage from Herod. ii, 77—"*ἐν γὰρ τῇσι μεταβολῇσι τοῖσι ἀνθρώποισι ἀι νοῦσοι μάλιστα γίνονται, τῶν τε ἄλλων πάντων κὰι δὴ κὰι τῶν ὡρέων μάλιστα.*" M. Boudin says, in his admirable 'Géographie Médicale' (Paris, 1843), that each country has its proper diseases, as it has its flora and fauna. The author of the treatise on 'The Humours,' not only asserts what is said above of the foretelling of diseases from the seasons, but the converse. Poor creatures that we are, we ought, says he, "to foretell also the coming seasons from the present diseases." I fear this may be my last reference to the interesting treatise on 'The Humours.'

[2] Here and elsewhere Hippocrates supposes also a fanciful relation between

"*Eastern* aspects are most healthy; there the temperature is even, and the morning sun disperses the mists. The people are of good complexion, clear-voiced, and sharp-witted. The productions of nature are also finer. The women are fertile, illness is uncommon, and such a climate is like perpetual spring.

"The reverse is true of *Western* aspects. The temperature is here variable, and damp dews alternate with burning heats. The inhabitants are sallow-complexioned and hoarse of voice, and they are liable to a great variety of disease. Such a climate is rather like perpetual autumn."

The subject of *Waters* is now separately discussed. "All *marshes, ponds,* and *stagnant pools,* are bad and bilious (χολώδεα). Those who drink them[2] have large and obstructed[3] spleens and dry and pinched bellies. Their faces and shoulders also are thin, the flesh going to feed the spleen. They eat much and are thirsty, because of their parched bellies. Frequent and fatal dropsies follow the obstinate quartans and dysenteries of the summer. In winter the young suffer from pneumonias and delirium;[4] the older from ardent fever. The women are liable to œdema and leucophegmasia. They are not fecund, and suffer from hard labours. They are liable also to dropsies of the womb, simu-

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the aspect and the qualities of the waters. The idea is not absurd, and he shows much sagacity in laying stress on the quality of water as a cause of health or disease. He could, however, use two fallible tests only, sight and taste, so that he unavoidably falls into much error of detail, and his remarks are necessarily of small value. M. Littré errs at times in drawing too much attention to the occasional truths of Hippocrates' facts. In so doing he may lead the unwary reader away from the real greatness of the master, which we see, not in his facts, but in his method.

[1] Here, again, I omit the influence of the waters, which are said, rightly or wrongly, to assist in forming the above constitution, their supposed qualities being fancifully attributed to the aspect, position of the sun, &c.

[2] Hippocrates had no knowledge of malaria, and he naturally referred the diseases which he observed to the drinking of the marshy waters.

[3] *μεμυωμένους,* interp. Daremberg sec. Hesych. "Hard" or "dense," Littré and Coray.

[4] *μανιώδεα νοσεύματα,* vid. Greenhill, note in 'Theophil.,' p. 185.

lating pregnancy. The children at birth are large and puffy, but wane and shrivel during suckling. They also suffer from scrotal swellings, as do their elders from varices and bad legs. In general, the people are short-lived, and old before their time."[1]

"*Waters which issue from rocks* are liable to mineral impregnation, and are hard and irritating, causing strangury and constipation; but waters flowing from grassy hill sides are soft and pure, though often so thin as to require the addition of wine." "All salt and harsh waters are bad, except as medicines in certain diseased states of body."

Hippocrates now amplifies his ingenious views concerning the influence of aspect upon the quality of waters. His opinions are often partially true; but, in the absence of chemical knowledge, are necessarily very imperfect. Snow-water he regards as unwholesome, as do many others.[2] Unaware of the purifying effects of evaporation, he conceived rain-water to be also unwholesome, not because it collects impurities as it falls, but because it is sucked up from heterogeneous and impure sources. Rain is caused, he says, by opposing currents of winds condensing the mists.

"*Rivers* also are an impure mixture of various waters, and their contents, when left to settle in a vessel, deposit a sediment. Those who drink of rivers are, therefore, liable to the stone, as also to strangury, sciatica, and hydrocele. Not that all drinkers will suffer alike. While men having cool bladders, free passage of urine, and open bowels, will escape, in others of the contrary habit a sediment will collect in the bladder, and soon concrete into a large stone. On making water this stone falls into the neck of the bladder and closes the passage, causing much pain, so that children pull and rub the penis, to the end of which they refer the seat of the evil. Unwholesome and heating milk, by decomposing the urine, causes stone in infants. Stone does not form so easily in young girls, whose short and wide urethra gives freer way to the contents of the bladder. They also drink more (!). Thus it is concerning these matters, or nearly so."[3] Nothing could show more clearly than the above paragraph the great disadvantage at which the Greek physicians were obliged to study disease.

"As regards the *Seasons*, if there be rain in the autumn, a tolerably cold winter, and an average rainfall in spring and summer, such a year will be wholesome. But if the winter be dry and northerly, and the spring rainy and southerly, the summer will bring fevers, ophthalmias, and dysenteries [vide Aph., iii, 11], caused by the blaze of a hot sun upon a moist earth and bodies laden with humours. If at the rising of the Dog Star there be rains and storms from the north-east the autumn may bring back health, otherwise it will be a deadly time for women and children, and convalescents will relapse into quartans and into dropsies. If the winter be rainy, southerly, and mild, and the spring northerly, cold, and dry, women will miscarry or bring forth weakly children, and the men will have dysenteries,[1] dry ophthalmias, catarrhs, or chest affections. Aged persons will die of hemiplegias. Towns on airy and sunny sites, and having good water, will suffer least. In dry summers diseases disappear, but in wet ones become chronic and tend to close in dropsies. Wounds then also have phagedenic tendencies. When summer and autumn are wet and southerly the winter will be unhealthy, and bring fevers to the old and phlegmatic, pleuropneumonias to the bilious." Hippocrates, in now describing the diseases which follow in dry and northerly summers with southerly and wet autumns, theorises according to his well-known humoral doctrines, but without that basis of sound observation elsewhere so apparent. In conclusion, he says, "In thinking over these truths we may see how to foretell the effects of the seasons. But we must be cautious at all moments of atmospheric change, and pursue no heroic treatment at the solstices or equinoxes, especially at the summer solstice and autumn equinox. Moreover, diseases have critical periods corresponding with the rising and setting of certain heavenly bodies."

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[1] Space prevents my proving the great sagacity of these remarks; and as I write for medical readers, I shall everywhere presume that the results of Hippocrates are duly appreciated by them without my assistance. I may say, however, that M. Andral refers to the reports of the physicians in the French expedition to Greece as curiously corroborative of the statements of the text. It will be remembered also that the translations, which I have made from the text, are necessarily very much condensed.

[2] Curiously enough, in the Tyrol, or parts of it, snow water is recommended as *not* predisposing to goitre, which is there attributed to the spring waters.

[3] More is said concerning the symptoms of stone in the bladder, and the detection of it by the sound, in the '*περὶ νούσων*,' bks. i and iv.

"I will now," he says, "make a general comparison between Europe and Asia, touching, for brevity's sake, on leading features only. Asia differs notably from Europe in the nature of all things, both in natural products and in the inhabitants. Everything in Asia is fine and large, the climate temperate, and the customs gentle; and this comes of the uniformity of the seasons.

"Asia has, of course, various regions; but the temperate parts are very rich in fruits, and are finely timbered; there is water in plenty, cultivation is easy, and all crops come to great perfection. The cattle are fruitful and improvable. The men are uniformly tall and well made. Such a country is, indeed, like a perpetual spring; but neither manly courage or perseverance, patience or vigour, can exist under such conditions, whether in natives or colonists, all being given to self-indulgence.[2] . . . . .

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[1] Haller demurs to dysenteries in spring; Coray, however, defends the statement of the text by supposing it to refer to some local peculiarity.

[2] Some suppose that a chapter on the Egyptians and Lybians is here wanting

"The people living on the right of the summer-sun rising up to the sea of Azoff are different from the preceding, their climate being variable and their soil less fruitful; for, in truth, it is with nature as with mankind—where the seasons are liable to sudden and great changes there the land is wild and rugged,[1] and he who has his eyes open will see the same to be true of the inhabitants. Some are like mountainous regions, moist and bristling with wood; others like dry and light soils; others, again, resemble marshes and savannahs; and others, bare and desert places. Let us take now marked instances, such as the Macrocephali, for example. With them nobility depends on length of head, and so arose the fashion of compressing the head in infants until the resulting shape, at first artificially produced, was in time established as an hereditary peculiarity, as the seminal fluid, which comes from the head as from other parts, was modified.[2] For if blue-eyed children come of blue-eyed parents, why not long-headed children of long-headed parents? The tendency is, however, now destroyed by intermarriage with other races. So much for the Longheads.[3]

"Those who live on the Phasis occupy a marshy country, which is moist, warm, and thickly wooded. They build their huts of wood and reeds in the midst of the waters.[4] They seldom use their legs, but paddle about in canoes, and they drink the stagnant and heated waters. Their vegetables are poor and watery, and do not ripen properly. The men are, in like manner, large, gross, indolent, and clumsy limbed. Their complexions are sallow, and their voices hoarse."

Hippocrates now returns to the Asiatics; and having shown that although their character, like that of their cattle and fruits, depends fundamentally upon their climate, he proceeds to point out that it is moulded also by their institutions. Being here less fettered by his ignorance of physical science, or rather his ignorance and our own being more on a level, we see Hippocrates in his real strength. The following masterly paragraph is among the finest of the kind in Greek literature:—"And as to the want of spirit and manliness in the inhabitants, the reason that the Asiatics are less warlike than the Europeans, and milder in their habits, chiefly lies in their seasons, which do not pass through great variations, either in the direction of heat or of cold, but are equable. There are, therefore, no terrors for the mind and no violent changes for the body. . . . . . For of all things it is vicissitude which most constantly stirs up the temper of man, and suffers it not to sink into ease. For these causes it seems to me that the Asiatic is a feeble race, and, moreover, also on account of their laws. For the most part of Asia is under despotic rule; wherefore, the men, having no power over themselves, nor being their own masters, but, on the contrary, enslaved, have, on this account, no reason for exercising themselves in the things of war, but rather for seeming unwarlike, as the risks are unfair; for it seems that they, indeed, must needs enter upon military service, and toil and die for their lords, far away from children, from wife, and from all other friends; and then, whatsoever good or honorable service they do, upon it their masters wax rich and great, whilst they themselves reap danger and death. In addition to all this, their land must become desolate by rapine and neglect; so that if any man be naturally brave and honorable, his disposition is thwarted by the laws; and there is a strong proof of this, for such Greeks or barbarians as in Asia are not under a despotism, but self-dependent and labouring for their own sakes, these are the most martial of all, for they encounter danger in their own cause, and do themselves bear away the prizes of their own valour, and in like manner the shame of their own disgrace.[1]

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[1] The cause is here put for the effect.

[2] This passage is in agreement with the treatise 'On Generation,' and with the long-accepted morphologic theory of generation rejected by Aristotle. Dr. Coray compares Hippocrates' theory of generation to that of Buffon.

[3] The existence of these Macrocephali, and their cephalic structure, with the causes thereof, are an old study of anatomists and historians. M. Littré, in the fourth volume of his edition of Hippocrates, gives Rathke's account of some such skulls which were discovered at Kertch. Dr. Davis tells me that since Rathke wrote similar skulls have been discovered in Austria and in other parts of Europe, and thought to be the skulls of Avars. Vide Fitzinger, 'Ueber die Schädel der Avaren,' 4to, Wien, 1858, which is the best memoir on the subject. On the Kertch skulls, Professor Von Baer's treatise is far the best—'Die Makrokephalen im Boden der Krym und Osterreichs,' St. Petersburgh, 1860, quarto, plates. The first plate is of a full-sized macrocephalic skull, found recently in the Crimea.

[4] Cf. account of Pæonians, 'Herod.,' v, 14. I need not say that the subject of lake dwellings is now exciting much interest. Such a mode of life is proved to have existed among certain nations from very early times to the present moment. Among the many authors referred to on this question I fail to remember any allusion to this passage of Hippocrates.

"There is in Europe a peculiar tribe of Scythians called Sarmatians, who live near the Sea of Azoff. There the women ride on horseback, use the javelin and bow, and take part in battle. They preserve their virginity until they have slain three foes, and after marriage they ride no more save in emergency of war. They have the right breast removed in infancy with a hot iron, lest it draw away nourishment from the shoulder. The other Scythians are a peculiar people, and are moulded by cold rather than by heat. The so-called desert of Scythia is a table-land, well drained and watered by large rivers. Here nomad Scythians dwell in waggons with four or six wheels, roofed with felt, and constructed as houses with two or three rooms. They are drawn by oxen, whose horns do not grow on account of the cold.[1] The women live in the waggons, and the men follow on horseback in charge of the flocks and herds, which they move about in search of pasture. They eat their meat raw, and drink the milk of mares, from which they make a cheese also. The region, being to the north, is cold and sunless; the seasons are, however, uniform, and so, therefore, are their bodily forms, the men being squat, flabby, and weak, and the women inordinately gross and fat. They have a way of firing their joints, so as to expel the moisture and make them sinewy; otherwise they could not draw the bow or hurl the javelin. A race of such lax fibre cannot be fruitful, and, moreover, much riding is not favorable to venery. A singular emasculation comes on in some of the men, and is seen in an effeminacy of voice and habits of life. This is said to be a divine visitation; but I believe it to be divine only in that sense in which all phenomena are divine; for no disease can arise save by natural causes.[2] The rest of the Europeans show great diversity of shape and stature, the seasons being very variable, and the spermatic fluid thereby subjected to changes of consistency. They are preserved from the suavity and indolence of the Asiatics by the continual restlessness of the weather, which conduces to bodily activity, courage, and enterprise. Nor are they enslaved by kings, as the Asiatics are, for people ruled by kings are necessarily poltroons, while those who are self-governed, as they meet danger in their own cause, so meet it willingly. Thus may we in a general way contrast Europe with Asia. In detail, however, it may be shown that the races of Europe differ much among themselves, those living in ill-drained districts having big bellies and spleens, and those who live on high and breezy lands being of different constitution; and so on. Of all influences the seasons are the strongest, and next in order the qualities of the soil and the water, the inhabitants of heavy moist soils with stagnant water being fleshy limbed, flabby, idle, mean-spirited, and sleepy, dense also and stupid in the practice of the arts, while the dwellers on rough bare lands are of dry habit, sinewy, and clean-jointed, sagacious, morever, in the arts, and fierce, vigorous, wakeful, and self-willed in temper. The same reasoning also may be applied to the natural products. So it is of extreme instances, and if we observe these carefully we may reason from them to all intermediate conclusions without fear of error."

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[1] Accurately put, the argument should be as follows:

1. Abundance of food encourages a numerous and an indolent population.
2. This makes wages low and wealth unequal.
3. Whence follows the irresistible domination of superiors.

I believe I have not trespassed unreasonably upon the reader's patience in giving a somewhat long analysis of this admirable treatise on ætiology. In it we see not only the finest treatise in the collection, but an example of the finest qualities of the Greek mind. Occupied as we well have been since the time of Bichat in working out details, it is not without advantage that we turn to such pages as these, looking, not for knowledge which it was out of the power of Hippocrates and his contemporaries to give, but for "vues d'ensemble," marvellous surveys of large groups of phenomena, the outlines of which are drawn with a truthfulness and a philosophic insight only reached in later times by such writers as Vico, Montesquieu, Comte, or Buckle. In the later middle ages we find men of transcendent powers of mind, who were, however, quite unable to sustain a steady and true line of thought without the continuous guidance of details; not having it, they fell blindly into absurdity or lost themselves in wasteful ingenuity. The mind of Hippocrates, on the contrary, was as true as a magnet. It is a curious fact that neither Montesquieu nor Buckle refer to this treatise of Hippocrates. It has been unfairly said of Montesquieu, that he owed much to Hippocrates that he never admitted; but in so far as the views of the latter on the power of political institutions in moulding the character of a people is true, so far it is likely that great men of other ages may repeat them. Moreover, Montesquieu would find at least as true a view of the above relation in the eighth book of the 'Republic' of Plato.[1]

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[1] Herodotus, in his celebrated account of the Scythians, makes the same curious error, and he is followed by Strabo. The horns were, in truth, cut off, as they are by the Tartars, who live in such waggons to this day. The desert here spoken of is probably the Ukraine.

[2] This affection has given rise to much curious speculation. The great interest of the passage to me, however, lies in the admirable protest of Hippocrates on behalf of positive science.

Aristotle's treatment of the subject in the 'Politics' (Bk. vii, c. 7) is slighter and less valuable than that of Hippocrates, and may well have been independent of it.[2] One remarkable fact concerning the treatise on winds, &c., has not, I think, been noticed. In common with many similar writings of the period, no allusion is made to the influence of religious systems upon the form and character of nations. One would have thought that material existed for such comparisons, the contrast between the Semitic and Aryan religions being one which throws a most curious light on the Greek temper in particular, and which must have been striking enough to any philosopher who crossed the Mediterranean or who read his Herodotus at home. This and other considerations of form and argument, upon which I cannot now enter, lead me to regard the present treatise as a fine fragment rather than as a complete work. However this may be, it is a work which, written in any age, must have testified to the splendid genius of its author, and which, amid the great literary works of its period, has an especial interest as showing a transition from the imaginative philosophy of the Asiatic Greeks to the more positive treatment of Aristotle. Like all that issued from the greater intellects of Greece, moreover, this treatise shows a marvellous comprehension and completeness of culture. Not only do we find a clever grouping of medical facts and a sound insight into the causation of disease, but we find a larger handling of the matter than this, a co-ordination of medicine with geography, history, and politics, which in its leading features requires little change at the present day.

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[1] Hippocrates, indeed, shows a truer conception of the dependence of man upon external nature than in many passages does Montesquieu. Montesquieu sometimes forgets that the influence of external nature upon man is to be studied, not in individuals, but in communities.

[2] It is quite certain, however, that Plato knew well the works of Hippocrates, and this treatise in particular. Comp. *e.g.* 'Phæd.,' p. 270:—"Hippocrates deals with the body as a generic total, with all its species and varieties, and as essentially relative to the totality of external circumstances," &c. &c.

To give anything like an adequate idea of Greek medicine in its several aspects, a short notice of some other and more purely medical treatises is absolutely necessary. Having, however, the fear of the Editor before my eyes, I must secretly implore my readers to stand by me if I put "To be continued" at the end of this article. Were I as tedious as a king, I could find in my heart to bestow it all on your worships; but yet some may say "Heu! quod tam pingui macer est tibi taurus in arvo," and how can I gainsay it? Others, again, may cry out with M. le Chevalier de Beaujeu, "Qu'est-ce-que nous avons à faire avec le temps passé?" "The time passed did belong to our ancêtres; very well, the time present is to us; they have their pretty tombs, with their memories and armorials all in brass and marbre. We have the *petits plats exquis* and the *soupe-à-chevalier*, which I will cause to mount up immediately."

Still, with a generous faith in the alliance, offensive and defensive, of my readers, this essay is sometime—

(*To be continued.*)

ART. II.

On the Reduction of the Subcoracoid Dislocation of the Humerus by Manipulation.

By ALEXANDER GORDON, M.D., L.R.C.S. Ed., Professor of Surgery, Queen's College, Belfast, and Surgeon to the Belfast General Hospital.

THE following simple, easy, and effectual mode of reducing the subcoracoid dislocation of the humerus I have now tried in nine consecutive cases. The facility with which the reduction was effected contrasts remarkably with the modes hitherto practised; and besides, this method is more in accordance with sound principles of surgery than most of the others.

If the right shoulder be dislocated, I place the patient on his back, with the shoulders raised, in bed, or on a mattress laid on the floor, or on a sofa. Standing on the same side and raising the elbow, I grasp the lower end of the right humerus; the thumb on the inner, with the fore and other fingers on the outer side, the fore arm lying flexed at an acute angle, resting on the web between the thumb and fingers. I raise the arm upwards and forwards, so as to place it at right angles with the surface upon which the patient is lying. Besides, to have complete muscular quiescence, I tell the patient to permit the extremity to rest upon and be supported entirely by my left hand. With the right hand I feel for the head of the dislocated humerus and press it downwards and outwards, either through the anterior wall of the axilla or in the axilla, moving, at the same time, with the left hand, the lower end of the humerus upwards and backwards, with rotation chiefly inwards.

Whilst thus engaged I have felt on several occasions a snap or jerk, so marked as to lead me to suppose, for the moment, that the dislocation was reduced. This snap or jerk is due to the head of the humerus having changed its position; for when we depress it we free it from the coracoid process, and the supra- and infra- spinati muscles, being on the stretch, jerk it outwards to the anterior border of the glenoïd fossa. When in this position, with the fingers in the axilla, I can feel almost the whole of the upper articular surface of the humerus, which I press outwards and forwards; or, in other words, I lift the head of the humerus over the inner margin of the glenoïd cavity, assisting with the left by rotation and very slight extension, if necessary, when the head enters the glenoïd fossa with a distinct snap.

The reduction of the subcoracoid dislocation of the humerus by manipulation resolves itself into three steps or procedures:

1st. Elevation and adduction of the arm, the forearm being flexed.

2nd. Depression of or freeing the head of the bone from the coracoïd process.

3rd. Lifting or pushing the head of the humerus forwards and outwards, so as to enable it to rise over the inner margin of the glenoïd fossa.

When we elevate the arm at right angles to the surface upon which the patient is lying, with the forearm flexed and supported, there is the greatest relaxation of the muscles which position can produce. The deltoïd is very much relaxed; the clavicular portion of the great pectoral, the biceps and coraco-brachialis, are relaxed. The only parts on the stretch are the lowermost fibres of the muscles forming the lower margins of the axilla. The supra- and infraspinati muscles are stretched, if not torn, by the displacement inwards of the humerus. Muscular quiescence is still further secured by the support given to the extremity by the hand of the operator at the elbow.

The second step in the reduction is to depress the head of the humerus, or to free it from the coracoïd process, by pressure through the anterior wall of the axilla or in the axilla. In this dislocation, before the head of the humerus can reach its abnormal position, it must describe a small arc of a circle around the coracoïd process, against which it is held chiefly by the supra-spinatus. By depression of the head of the bone, and by moving its lower end upwards, outwards, and backwards, with rotation inwards, we disengage the head from the coracoïd process, and the moment we do so the stretched spinati muscles jerk it outwards and hold it against the inner border of the glenoïd cavity.

The third step mainly consists in raising the head of the humerus forwards and outwards, so as to enable it to rise over the inner margin of the glenoïd fossa. This in many cases can be easily effected by pressure alone upon the head of the humerus in the axilla; in others by assistance with the left hand, which grasps the lower end of the humerus. Or slight extension from an assistant may be necessary, as in two cases of subclavicular dislocation which I lately met with, in which, when the arm was elevated, the head of the humerus was too deeply buried in the subscapular fossa to enable me to get my fingers beneath it.

It should be remembered that in this mode of reduction the chief resistance experienced in freeing the inner margin of the glenoïd cavity arises from the muscular fibres forming the lower margin of the axilla. The resistance offered by these fibres may be overcome by lowering the lower end of the humerus with the left hand, whilst we press upon the head of the humerus in the axilla with the right. Indeed, the whole procedure may be said briefly to consist of manipulations which enable the stretched spinati muscles to reduce the dislocation.

If we glance at the records of cases of reduction of dislocations of the shoulder, we often find it stated that, after making considerable extension unsuccessfully, the head of the bone has, from some casual movement, glided quite unexpectedly into its cavity. This occurred in a case in which I had made considerable extension. The patient, a female, being partially under the influence of chloroform, began to scream whilst I was making extension. I therefore desisted until more complete anæsthesia was induced, and proceeded in the mean time, the arm being raised, to determine the precise situation occupied by the head of the humerus, and in doing so it glided into its place.

On reflecting on these circumstances, I determined to imitate the steps which unexpectedly led to the reduction in this case in the next that presented itself to me, and on doing this the reduction was effected with the greatest ease and without pain.

There may be circumstances connected with this mode of reduction which I have overlooked or misinterpreted, yet the probability of reducing dislocations in this simple manner clearly points to the necessity for closer examination of individual cases.

Although I have described this mode of reduction as especially applicable to the subcoracoïd dislocation, I am convinced that it will be found almost equally successful in the other forms. In the last two patients whom I saw labouring under dislocation of the humerus the head of the bone lay internal to the coracoïd process, and almost an inch below the clavicle. I raised the arm as usual, and depressed the head of the bone. It was deeply lodged in the subscapular fossa, though not so low as the lower margin of the glenoïd fossa. Neither by pressing the elbow outwards with the left hand nor with the right in the axilla, could I raise the head of the bone sufficiently to enable it to free the inner margin of the glenoïd cavity. I therefore directed one of the assistants to pull gently upon the arm; and a very moderate extension indeed enabled me to raise the head of the bone and restore it to its place.

If I might be allowed to hazard an opinion from the experience of eleven cases of dislocation of the humerus occurring consecutively, I should say that the resistance to the reduction is due rather to the fibrous structures than to muscular tonicity. If we extend slowly, gradually, and gently, for some time, the resistance from muscular tonicity alone will cease; and if we cannot reduce the dislocation it is because the fibrous structures around and within the muscles oppose us.

Art. III.

On the Influence of Age in Hereditary Disease. By William Sedgwick.

As the present subject of inquiry forms the second of a small series of three papers which have been designed to illustrate in succession the influence of sex, of age, and of atavism in controlling hereditary transmission, it is proposed to offer a few remarks on this division of the inquiry into three parts before submitting the evidence on which the present paper is based.

It appears that, until comparatively a recent date, all inquiry on the subject of hereditary transmission in disease was directed to the accumulation of cases for the simple purpose of establishing the fact that disease may be transmitted by parents to their offspring as an inheritance. The evidence collected by successive observers in support of this fact is now so abundant as to be greatly in excess of what is needed for its proof; and, therefore, in accordance with the utilitarian opinions of the present day, instead of going at random again and again over the same ground, efforts are being made by myself and others to impart precision to the inquiry, in the hope of being able by this means to direct its course to some useful result. The need of precision both in observing and in recording cases of hereditary disease is obvious even at the outset of the inquiry, and it cannot be too strongly urged on all who would desire to take part in this investigation; for without a firm groundwork of well-observed facts it would be useless to expect to make any satisfactory progress. It seems, indeed, to have been chiefly in consequence of this want of precision that so many of those who have on various occasions been attracted to the subject, and who have sought to know something more than the bare fact that disease is sometimes hereditary, have for the most part quickly turned aside from the pursuit, lest amidst the apparent contradictions and obscurities of transmitted disease they should perchance lose their way.

In commencing, moreover, an inquiry of which as yet the end cannot be known, and in pursuing which there is a possibility of going wrong, it has been considered necessary that there should not only be a large stock of suitable cases at starting to supply what may be wanted during the progress of the inquiry, but also that convenient stations should be fixed upon whereat to rest, as it were, on the way. Such resting-places have been provided by dividing the inquiry into three parts—namely, sex, age, and atavism; sex referring to the source of the disease, age to the period of its development, and atavism to the interruptions in its transmission: and it is, perhaps, almost unnecessary to state that whilst each of these has a distinctive importance of its own, the influence of all three is occasionally combined in the same hereditary disease.

With respect to the controlling influence of sex, which was selected to be the subject of the first paper in this series in consequence of its referring to the source of hereditary disease, and therefore having a prior claim to consideration in all questions concerning reproductive development, it was proved at that stage of the inquiry, that with reference to the reproduction of disease, in whatever way the transmission may be effected, there is a tendency to limit its development to members of one sex in the family affected, and that this limitation by sex occurs both as an independent condition, and also in connection with limitation by age and by atavism.

The time which has elapsed since what may not improperly be called a good position was secured at this first stage of the inquiry, has been to some extent occupied in opening up communications in advance; and as a fresh supply of suitable cases and observations has been obtained, it is now allowable to hope that an equally good position may be established at the second stage of the inquiry, by proving that age, like sex, though usually in a less degree, has also a controlling influence in hereditary disease.

Before, however, attempting to illustrate the influence of age in hereditary disease, it will be necessary to notice some of the misapplied terms which are now commonly employed in connection with the subject, and which have a tendency to misrepresent the nature and extent of this influence on reproductive development. The necessity, indeed, of some preliminary notice of this kind on the present occasion is very great; for it will unfortunately be found, as the inquiry proceeds, that the pathway is obstructed by various delusions which may often be traced to verbal deceptions, and that, in consequence of what appear to be fundamental errors in observation, many words have slipped into use which have had a pernicious effect on the language employed. A careful investigation of the influence of age will in this respect be useful in showing that the frequent recurrence of some of these inappropriate words undoubtedly results from deeply-seated errors regarding hereditary transmission. For example, the words predisposition and latent, both of which are in common use and are specially connected with the present inquiry, appear to be more than usually inappropriate when employed, as is very often the case, to express or to suggest a denial of the relation subsisting between the material organisation and the so-called vital force on which its development depends. Attempts have been repeatedly made by successive writers on the subject either to establish or to imply that there is an antagonistic distinction between hereditary disease and the predisposition to disease; and these attempts have not merely led to much confusion on the subject, but have in other respects greatly hindered the progress of the inquiry: in consequence of which they may be considered together as widely spread errors, which, to continue the simile used at starting, have served to block up the way like a turnpike-gate. For neither the use of the word predisposition in opposition to hereditary disease, nor the frequent recurrence of the word latent, is in accordance with any well-observed facts in the hereditary transmission of disease. The long and profitless discussions which have been indulged in, as to whether in hereditary transmission it is the disease or only the predisposition to disease which is conveyed from parents to their offspring, would seem to have led some writers on the subject even to deny the reality of a morbid inheritance; whilst amongst those who believe in the transmission of disease there is not only a great difference of opinion, but also some confusion of ideas. Part of the difficulty in this case is evidently due to the different meanings which have been attached to the term predisposition; but, taking it in the sense in which it is commonly employed, it will be found as one of the results of the present inquiry, that the much-cherished theory of hereditary transmission, which assumes that as coming events cast their shadows before, so the child inherits the shadow of what in the parent was a disease, crumbles beneath the influence of age. And so, too, the term latent, in connection with hereditary transmission, cannot now be regarded otherwise than as the expression of a once popular delusion, which seems to have been founded on the supposition that, in some mysterious manner, either the disease itself, which had been transmitted, could be kept for a certain time concealed, or that the abnormal exercise of generative force on which the morbid development depends could be prevented for a certain time from proceeding any further in the wrong direction. In consequence of which, it has been alleged that when in the case of hereditary disease the parent is reproduced in the offspring, although the corresponding development in their organisation will, as a rule, be observed to occur at corresponding periods of their lives, yet, in accordance with this delusive theory, it has been very generally supposed that the influence of age can be easily disposed of by stating that whenever an hereditary disease is not apparent, it is latent or concealed. Whether it might or might not be possible to assign any limits to this latent condition in disease, does not, for the most part, appear to have troubled the minds of those who have used the word, for the subject is commonly referred to as if it were a game of hide-and-seek. But it may be instructive to notice that all have not in this respect been equally content; for one writer on the subject, who seems to have wandered very far from the right track, has endeavoured to show that hereditary disease is due to the latent mischief which resulted from breathing "mephitic vapours" during the deluge, when our ancestors were sheltered in the ark.

In like manner, the term vital force has often been misapplied, so as to convey the impression that although at the beginning of individual life the material organisation exists in a very rudimentary form, yet the immaterial principle residing in it, so far from being equally rudimentary in its earliest condition, exists in more or less full perfection from the first, as though the germ were to be regarded as a condensing apparatus, and life as the process of letting off the steam. Even the author of the graceful article on "Age" in the 'Cyclopædia of Practical Medicine' is inclined to adopt this opinion, and suggests "that a certain stock of vital force is imparted to the embryo at its first formation, as a provision for carrying it through its destined career of existence. In every action of the system a portion of this power is expended, and the greater the expenditure the less must there be remaining, till, at length, the whole being consumed, all movements cease, like those of a watch which has run down, and of which the mainspring has ceased to act." The stoppage which is referred to in this case appears, however, to be due to something more than the consumption of a certain stock of force; and the writer, in adopting this popular simile of a watch, would have done well to remember that as a watch may be warranted to keep time for a certain number of years, in accordance with the relative soundness of its mechanism as a whole, and the relative durability of its component parts, so likewise it might be inferred that the body could be constructed to last free from disease for a certain number of years, and then pass into a morbid state, in consequence of some tissue or organ of which it is composed being able to keep free from abnormal change only up to a certain age. In this respect the regularity of the sequence as regards age which is observable in many cases of hereditary disease may to some extent be illustrated by means of a watch; although, perhaps, as an example of the correlation which exists between the exercise of organic and physical force, it might be more suitably compared with the calculating machine of Mr. Babbage, in which the mechanical process of counting goes on in a regular manner till a certain limit is reached, when the orderly succession of numbers is lost, and disorder is observed to begin. At the same time, also, it should be remarked that whilst it cannot as yet be said on what the mechanical disorder in the working of the calculating machine depends, yet, as regards disease, which in relation to health illustrates a corresponding abnormal change, a rational explanation can be given. For when a tissue or organ becomes at any particular age the seat of hereditary disease, such an occurrence proves that it has retained through all its changes a true record of the past; and it illustrates very forcibly the correctness of Hunter's definition of habit as the "memory of the body," which is here shown in the recurrence of hereditary disease limited by age.

In offering these prefatory remarks on the misuse of words in connection with the development and transmission of hereditary disease, there is no intention to say more than the occasion requires; and it is chiefly in consequence of the subject being closely connected with the influence of age that it is now noticed. There is, indeed, in the present day an urgent necessity, before proceeding far in any inquiry, to protest against such verbal errors, since, as obstacles in the way of scientific progress, they are far from uncommon; and although it has been customary of late years to speak of the obstructive tendency of hasty generalisation, yet this is probably a less evil than the improper use of a word as a stop-gap. For there is a large class of observers who are very apt to suppose that whenever a difficulty occurs which at first sight cannot be overcome or removed, it is better, instead of passing it by and leaving it to be encountered by subsequent inquirers, to dispose of it at once; and this, they seem to think, can be accomplished by having recourse to the simple contrivance of substituting a word in the place of further investigation. In accordance with this plan, the terms latent and catalytic have been in popular use amongst chemists, and functional has been in like manner applied to disease, without in either case conveying any satisfactory information respecting the nature of the chemical or the morbid changes to which they refer. And, in like manner, the misuse of such terms as predisposition and latent in connection with hereditary disease has been attended with much mischief, in consequence of their tendency to arrest inquiry where there is most need of further information as a guide. Every attempt which has been made to set up predisposition in opposition to hereditary disease appears to have been based on imperfect information respecting the influence of age in reproductive development; and as, strictly speaking, all transmitted disease, whether congenital or not, is the result of the predisposition which is inseparably associated with the material organisation throughout every period of its life, no one who had not been previously accustomed to place undue reliance on words possessing when so used, to say the least, a very doubtful meaning, could seek to establish an opposition between the inheritance of a predisposition and the inheritance of a disease. The application, also, of the term latent to signify that an inherited peculiarity or disease may exist for any given number of years or of generations in a concealed state of quiescence, so that whilst development is going on in all the normal structures around it, that only which is abnormal can remain unchanged, is merely an idle supposition, in many respects opposed to what has been recognised to occur in the case of non-hereditary disease, and, like the misuse of the term predisposition, may be referred to error respecting the influence of age. Hence, as the inquiry advances, it will be perceived that not only is there no necessity for thus endeavouring to explain the occurrence of delay in the appearance of transmitted disease, but that such an attempted explanation would be contrary to what is known on the subject of reproductive development both in health and in disease; and that it would, moreover, ignore the influence of age on the development of that generative or so-called vital force on which life depends, and whose changes are correlative with the organisation in which it dwells. For, unless there was this parallel progress in the development of both, the rule by which the organisation reflects the image from which it is derived would cease to be maintained.

With respect, therefore, to the influence of age on the development of hereditary disease, it may not be unreasonable to assume, from what is already known or can be safely inferred on the subject, that even with the earliest dawn of individual life there is present in the germ a generative force on which its development depends; and whether such development agrees with the ordinary law of life as regards the race to which the individual belongs, or differs from it at any period of its existence in consequence of some imperfection in the source from which it was derived, it is evident that unless this force be altogether independent of the organised structure with whose changes it appears to be inseparably combined, it must itself pass through a developmental series of changes, and be subject in consequence to the same influence of age. Before assuming the contrary, it would be necessary to disprove the apparently intimate connection which everywhere prevails between force and matter in organic development; and as there is no evidence even to justify the inference that it is possible during life for the one to have an independent existence apart from the other, it may be urged with reference to hereditary disease, that it would be quite as unreasonable to expect that the products of abnormal development—such as tubercle, cancer, and other results of diseased action associated with the blood and deposited in the lungs or elsewhere—should be present in the germ, where as yet there is neither blood nor lungs, as it would be to expect the pre-existence in the germ of a complete set of bones, muscles, or any other structures peculiar to an after period of life, and which common observation has shown to be the result of successive changes which must, as a rule, intervene between their formation and that of the germ in its first or elementary form. Moreover, the analogy of the mind in its relation to the progressive development of the brain offers a corroborative proof that the force on which the evolution of the germ in each case depends must pass through a progressive series of changes which are inseparably linked in each successive stage of evolution with the body in which it is lodged, and which cannot, therefore, be otherwise than of the simplest character in the first instance, whilst its subsequent development must be in harmony with that of the body, whose form is the expression of its influence, up to the latest period of life.

With regard to the application of the term "age" in hereditary disease, it may be here observed that its use will not be restricted to the interval of time between birth and death, but that it will also be employed to mark the duration of fœtal life; for as limitation by age is dependent on progressive changes in development, it is evident that the organisation must be liable to be affected by it during the whole period of life, from conception to death. The evidence which will be adduced in favour of this extended range of influence will, for the sake of convenience, be arranged in two divisions; on the one side the proof being derived from cases in which the disease or peculiarity has been developed before birth, and on the other side from cases in which its appearance has been subsequent to birth. It is, perhaps, in some degree difficult at starting to recognise this in theory as the possible range of the influence of age; but in the course of the inquiry it will become obvious, that as disease may at any time be associated with development, and consequently limit or disturb any one or more of those successive changes which commence with life and cease only at death, birth can simply be referred to as a notable event in the history of hereditary disease, like the "coming of age" in the history of an individual life. The proof of this wide extent of influence will therefore be derived from a series of carefully recorded cases, in which it will be noticed that there is in some families a partial limit or disturbance in the progress of formation during fœtal life, resulting in those abnormal forms of development which are commonly described as congenital deformities, and which, when presenting the same characters, may be referred to the same fœtal age; and that there are also certain morbid conditions or diseases of the fœtus which are associated with hereditary influence, and which often result in its death and expulsion at some fixed and recurring period of intra-uterine life; whilst with regard to cases occurring after birth, besides those hereditary diseases which are restricted in their appearance to one age, either from preference or from acquired habit, there is in some families an hereditary limit to life, consequent on the development of the same or even of any disease the occurrence of which is fatal at a certain age.

It is perhaps almost unnecessary, after what has been already stated on the subject in connection with the influence of sex, to dwell again on the distinction between preference and acquired habit in development. There can be no doubt that, with reference to the occurrence of disease as a family defect, in addition to what may sometimes be ascribed to preference, there is frequently also a restrictive influence in age, which, from its accompanying transmitted disease, appears to be acquired by inheritance. The influence of preference in limiting disease to a certain age is shown in those cases in which the disease is usually associated with one period of the air-breathing life, such as infancy, childhood, puberty, and the climacteric change in both sexes; and when such diseases occur hereditarily, they are in consequence almost necessarily limited to that age for which they have a natural preference. In like manner, also, a large proportion of those peculiarities and defects which are congenital, are limited by preference for one age, when, as arrests of development, they can only be referred to one period of fœtal life; and their chief importance in the present inquiry is due to the exactness of their hereditary repetition.

But, apart from this natural tendency in certain diseases and defects to appear at one period, whether of fœtal or of air-breathing life, it may moreover be noticed, that when disease is developed in connection with heredity, especially during the air-breathing life, there is frequently a well-marked limitation by age which is independent of any preference for a particular epoch in the development of the body; the recurrence of the disease in such cases being regulated by the age at which it first attacked the ancestor from whom it was received, or the first member of the family to whom it could be traced; and even if this has happened to be at some exceptional period of life, the disease is still apt to be restricted to that age when it subsequently reappears either in the succeeding offspring, or in the more remote descendants. It will, perhaps, without much hesitation be acknowledged, after a careful examination of the following observations and cases, that limitation by age in hereditary disease is, strictly speaking, nothing more than an illustration of the influence of time on development, and that it is one of the results consequent on the changes occurring in the system being progressive. If, on the one side, it were possible to suppose that the organisation could for a given number of years continue at rest, or without undergoing any progressive change, there would not be during such a period any development of hereditary disease: whilst, on the other side, the occurrence of a disease or defect at the same age in successive members of the same family is a strong evidence in favour of its being hereditary, instead of being due to endemic or other external influence; which simply tends to promote the abnormal development on such occasions, in the same way that the use of manure in seed-time is found to promote the harvest. For as in all natural development there is, broadly speaking, continual reproduction—the model being the ever-changing form of a preceding generation, in consequence of which the resemblance between offspring and parents is, as already stated, most strongly marked at corresponding periods of life—it may be inferred as a necessary result of this law of development, that there is a similar tendency to limitation by age in the case of hereditary disease, and that its reproduction is, within certain limits, an allotted work of time. If, therefore, it can be admitted that the principle of action in the hereditary transmission of disease is the same as that which prevails in ordinary development, and there is no evidence to the contrary so far as the influence of time is concerned, it becomes obvious that a man who has had his system modified to such an extent as to suffer from some permanently organic change at a certain period of his life, is not only liable to transmit the same to his offspring, supposing that they more or less closely resemble him in organic development, but that, in consequence of the change having affected him at a certain period of his life, the liability of his offspring to the disease is greater at the same rather than at a different age.

With regard to any suitable division of the inquiry it must be admitted that although strictly speaking no clearly defined boundary can be traced between the various cases which may be cited in favour of this influence in morbid development, yet, in order to prevent confusion, it will be convenient to regard them as separated by birth into two large divisions—namely, the congenital and the non-congenital—each of which will consist of distinct groups of cases, arranged to illustrate, as far as possible, the influence of age at successive periods of life.

In the first of these two divisions will occur those congenital peculiarities and defects which result either from arrest of development or from some other disturbing cause; and they generally exhibit very distinct evidence of the influence of this hereditary restriction. It is not to be expected that the effect of limitation by age can be detected at the first glance, or be recognised with equal facility in all cases of this description, whether they result from arrest, inequality, or excess of development. On careful examination, however, it will be found that all congenital defects which are expressive either of deficiency or of excess in the organisation may usually be referred to definite periods of fœtal development; and, for the most part, they afford excellent illustrations of the restrictive influence of age. For as the effects of this influence can be most readily traced where the abnormal change has left most distinctly its mark, they show, like the boundary-lines left by the tide, how far each wave of development has advanced. In this respect they serve, each in its turn, as a self-registering index of the age at which the abnormal change of structure began which led to the occurrence. This, as a rule, can be readily observed in cases more especially of deficiency of development; and such cases could, for the most part, without difficulty be chronologically arranged so as to mark the progress of development during fœtal life.

In cases involving deficiency or absence of structures at a distance from the central organs, and consequently of no great importance to life, there is often to be noticed a very decided tendency to their repetition in families, which to some extent increases in proportion to their remoteness from the centre; and this is very strongly marked in defects involving excess as well as deficiency of the peripheral structures, such as the fingers and toes, and the skin with its appendages, including the organs of special sense. In many of these cases, which some observers might be inclined to regard as less instructive than curious, there is often much to be noticed which is at first sight apt to be overlooked, but which, on further inquiry, will be found to be relatively of great importance in serving to illustrate the restrictive influence of age; for they show that, however trivial may have been the cause which in the first instance led to their occurrence, yet in becoming hereditary they have retained through successive repetitions essentially the same abnormal character. This exactness in the recurrence of a defect is apparently due to the organisation having possessed a very unerring memory of the time when the exceptional development should in each case commence, and consequently the completeness with which a defect is repeated in successive members of the same family would in this respect be an evidence of the strictness of its limitation by age; for if an arrest or an abnormal change of development occurred earlier or later than the appointed time, the resulting deformity would, as a rule, present a corresponding deviation from the preceding family defect, instead of being, as is frequently the case, an exact copy of it. This evidence in favour of the restrictive influence of age is sometimes very noticeable in cases in which it could be least expected to occur; as, for example, in many cases of digital deformity, in which a very close adherence to an abnormal type is the more remarkable, as such defects, like the sporting varieties of the leaves of plants, seem to be, at least in some instances, the result of what may be regarded as a natural tendency in animals as well as plants to vary; and consequently cases of this description would tend to prove that the restrictive influence of age is sometimes stronger than other opposing influences in development.

There is, perhaps, more useful information to be gained in tracing the hereditary transmission of these trivial defects than in observing others of a more abnormal character; and consequently, before quitting this part of the subject, it will be desirable to cite the two following cases as illustrations; one of which is a case of symmetrical deficiency of the middle fingers and middle toes, which, although advanced, somewhat hastily, by Mr. Paget as evidence in favour of deformity caused by maternal impression, appears to be more noteworthy as an instance of the hereditary influence of age; whilst the other is a carefully observed case of a limited form of palmidactylism. With reference to the first of these two cases, which is the one recorded by Mr. Paget,[1] the middle fingers of both hands and the middle toes of both feet were absent in the first and sixth children of a woman who is said to have suffered during these two pregnancies from the same mental impression. The four intermediate children were free from the deformity, the mother having been during these pregnancies free from the mental impression to which the defect in the other children was referred. In this case it is evident from the symmetrical character of the deformity, and from the exactness observed in its repetition, that the abnormal arrest of development must have been strictly limited to the same period of fœtal life; and consequently, in referring its occurrence to maternal impression, it would be necessary to assume, not only that the impression was precisely the same in each case, but that it occurred at the same period of pregnancy. However remarkable such a coincidence might appear, it may notwithstanding be admitted that it is not altogether impossible; but before having recourse to an explanation founded on evidence which is almost necessarily of a very questionable character, it may be useful to notice that there are in the records of hereditary defects a very large number of corresponding cases in which the exactness of the repetition has been due to hereditary influence, altogether apart from maternal impression; and that the evidence in favour of the assigned cause in this case would have been stronger, if the repetition of the deformity had been less complete. For the impressions experienced by pregnant women are characteristically very capricious; and if they do occasionally recur in subsequent pregnancies, there is seldom if ever any approach to that uniformity which is noticeable in periodic phenomena, and which is often well illustrated in the atavic transmission of disease. It has become the more necessary to notice the paramount importance assigned to maternal impression in this case, as there seems to be an increasing tendency in the present day to refer congenital and especially digital deformities to this cause; and even where it is not asserted to be the primary cause of the defect, it is sometimes supposed to be the cause of its recurrence. Dr. Struthers,[1] for example, is of opinion, that in some of the cases of digital variation appearing in more than one member of the same family, and where no previous history could be traced, "it might be held that the first case having occurred spontaneously, the circumstance had operated by an impression on the mind of the mother," and so led to the reappearance of the defect in some of her other children. This attempt to refer digital and other defects to mental impressions seems to show a desire to get rid of a difficulty without being subject to the obligation of explaining it. Mental impressions may and probably do influence development, but not to the extent which is commonly supposed; and one great cause for the popularity of this idea is, no doubt, owing to the fact that some excuse is supposed to be needed for a woman having imperfect offspring; and therefore, whether she is or is not more susceptible to mental impressions during pregnancy than at other times, this is thought to be the best apology that can be offered for the occurrence. But if attention be turned from this popular notion to the plain facts of scientific observation, it will seldom if ever be found necessary to have recourse to so doubtful a theory as this. For, independent of the fact that many cases of abnormal development are due to the exclusive influence of the male, and that the female is merely a medium of transmission, there is a very large number of cases in which, from the exactness witnessed in each repetition of the deformity, it may with reasonable certainty be referred to hereditary influence.

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[1] 'Medical Times and Gazette,' September 23rd, 1865, p. 333.

In the following case, which was lately brought before the notice of the French Academy of Sciences by M. Berigny,[1] and which is the second of the two cases referred to above, it will be noticed that the same exactness of repetition occurred both in atavic and direct transmission, and equally so whether the defect was reproduced on the fingers or the corresponding toes, which are homologous structures; and it offers, in consequence, a convincing proof of the influence of age in the limitation of abnormal development. In this case, "the third and fourth toes of the right foot of a woman were joined together throughout their entire extent, the hands and feet of her husband exhibiting nothing abnormal. She had seven children, none of whom presented their mother's deformity. In the third generation, however, one of her daughters had, among other children, a girl who exhibited a palmated condition of the medius and ring fingers, just like the toes of her grandmother. Another sister also had, among her other children, a boy and a girl both with the medius and ring fingers palmated. Of three brothers of the two above-named sisters, one had, among five male children, his eldest with fingers exactly like those of his cousins; making altogether four children in the third generation who inherited the abnormal digitation of their maternal grandmother. In the fourth generation, one of the grandchildren, the eldest of the boys with the palmated fingers, became the father of female twins, one of which had her toes united exactly like those of her great-grandmother, and of a boy who presented the same peculiarity of the fingers as did his father. The case is further remarkable, inasmuch as the anomaly was always witnessed in the eldest children, and always on the right side."[2]

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[1] "On Variation in the Number of Fingers and Toes, and in the Number of Phalanges, in Man;" 'The Edinburgh New Philosophical Journal,' 1863, p. 104.

In like manner, many hereditary peculiarities in the form and mobility of these structures may be cited as illustrations of the restrictive influence of age; for it will usually be found that the exceptional development on which the peculiarity depends, and which must necessarily have been limited as regards the time of its occurrence, is very closely repeated in the different members of the family affected; so that, whether it occurs in a little finger or in a thumb, the result as regards its situation and its character is the same as in the preceding cases of hereditary deficiency and excess. This is well shown in a case which has come under my observation, in which a family consisting of three sons and three daughters all had reversible thumbs, or the power of bending back the thumb so as to allow the tip of the thumb-nail to touch the skin over the base of the corresponding metacarpal bone: the peculiarity, which was inherited from the mother, was symmetrical in its occurrence. In addition to cases of this description, there are on record many well-marked examples of distortion affecting a single finger, some of which have been already noticed in connection with the influence of sex in hereditary disease; and it will, therefore, be sufficient on the present occasion to cite merely one more case, recorded by Dr. Struthers,[1] in which three brothers had a peculiar curve of the little finger; and their paternal grandmother and some of the children of a paternal uncle had "exactly the same deformity."

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[1] 'Medical Times and Gazette,' November 14th, 1863, p. 520.

[2] With reference to the invariable occurrence of the digital defect in the eldest children in the above case, it should be observed that a case was brought before the notice of the French Academy in the following year by M. Chassinat ('Archiv. Gén. de Méd.,' Janvier, 1865, p. 104), in which a man with supernumerary fingers and nails transmitted the same peculiarity to his second and third children, whilst his first and fourth children were free from it. This, like the preceding and many other cases, appears to have been simply an instance of hereditary restriction as regards the period of its development, in consequence of which the defect was either exactly repeated or completely absent in the offspring. It was cited, however, by M. Chassinat to show that there is not only an habitual resemblance between a mother and her first child in normal, but also in abnormal development; and that the freedom of the eldest child from the defect in his case is referable to this rule in hereditary transmission. But no such rule has yet, nor indeed is likely to be established, for the tendency of pathological inquiry on this subject is to prove that the female is for the most part merely a medium for transmission in hereditary disease, which is not limited to her own sex; and consequently neither the physical development of the mother, nor the emotional affections of the puerperal state, can rightly be supposed to have any such overpowering influence on the offspring as is commonly assumed to be the case.

Similar illustrations of the restrictive influence of age are presented in the hereditary transmission of many other congenital defects. The absence, for example, of nails, may in some cases be the result of hereditary deficiency in the appendages of the skin, independent of any such deeply seated defect as occurred in a case which came under my own notice, in which it was connected with congenital absence of the corresponding phalangeal bones. A case of this description, in which there was hereditary absence of all the nails, associated with congenital baldness, has come under the observation of Dr. J. W. Ogle, who has kindly sent me his notes of it. The case is that of a man who died lately, at the age of sixty years, completely bald, and without nails from birth. His father, paternal uncles, and paternal grandfather were all, like himself, bald and without nails from birth; and he had an only sister who was also bald and without nails from birth. He left two sons and a daughter, of whom the eldest son had no peculiarity, but the other son, aged fifteen years, and the daughter, who was accidentally burnt to death, were both born without either nails or hair. Whilst Froriep[2] has related a case of congenital alopecia, without deficiency or absence of the nails, in two sisters in whom the hair was not developed. The congenital peculiarity in these cases, which in that supplied by Dr. Ogle was observed to be hereditary for four generations, corresponds with those cases of hereditary deficiency in the appendages of the skin which have been sometimes observed among the lower animals; as in a case recorded by Otto,[1] in which there was an hereditary absence of feathers in a race of doves, which was maintained in this featherless condition for many generations; and with those cases in which horses and dogs have in like manner been characterised by an hereditary absence of hair.[2] It is evident that the hereditary peculiarity is equally indicative, on all these occasions, of the restrictive influence of age; for when the arrest of development both as regards situation and extent is the same in each member of the family affected, its occurrence must be assigned to the same date.

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[1] Op. cit., p. 90.

[2] 'Dict. Encyc. des Sci. Méd.,' art. "Age (Médicine légale)," tom. ii, p. 150, 1865.

It would be instructive also to notice, if our space allowed it, some of those hereditary defects which are more or less strictly localised in other organs or tissues; for the more closely such limitation is maintained, the greater will be the exactness observed in the date of their occurrence: such, for example, as the hereditary cases which have been recorded by Sir Henry Holland[3] of a singular elongation of the upper eyelid; and by Mr. Harrinson,[4] of congenital ptosis. The great exactness of repetition which is not unfrequently maintained in many cases of hypospadias, is also indicative of the restrictive influence of age. This was specially noted in a case recorded by Belloc,[5] in which a man at Agen, who had the orifice of the urethra at the base of the frænum of the glans, left four sons, two of whom had the same malformation. Illustrations of an equally direct transmission of the same form of hypospadias are not uncommon; for Simeons of Offenbach[6] remarks, "My observations refer to eight hypospadiacs, of which the first and the second are married. Both have been for a long time under my observation; one of them has six children, and the other has four... The third and fourth hypospodiacs are two brothers; the fifth and sixth are sons of the first. The seventh is remarkable, among other things, on account of having been the object of a consultation tending to provoke a divorce. Lastly, the eighth is an infant. In all these hypospadiacs, the urethral orifice was not situated in the gland, but behind the attachment of the prepuce, in a line with the urethra." Other writers on the subject have related corresponding cases in which hypospodias was directly transmitted from father to son; and it is probable that the extent to which the defect is liable to become hereditary is much greater than might be expected, considering its nature; since even when the malformation is so great as to lead to impotence, and to the consequent impossibility of reappearing in direct descent, yet its transmission as a family defect is far from

[1] 'Pathological Anatomy,' transl. by John F. South, p. 121, 1831.

[2] Ibid., p. 120; and Girou de Buzareingues, 'De la Génération,' p. 120, 1828.

[3] 'Medical Notes and Reflections,' 3rd Edit., p. 33, 1855.

[4] 'British Medical Journal,' December 15th, 1860, p. 979.

[5] Quoted by Dr. T. E. Beatty, 'Cyclop. Pract. Med.,' vol. ii, p. 596.

'Dict. des Sci. Méd.,' tom. xxiv, p. 202; 1818.

being on this account always checked; for by means of atavism, assisted by what appears to be on some occasions endemic influence, hypospadias has occasionally reappeared in the indirect descendants of the same family, and has been found to prevail as a family defect in certain districts. With regard to the occurrence of atavism in connection with this limited defect, and the consequent association of the three influences of sex, age, and atavism in the same case, Heuremann[1] mentions the case of a family the females of which had for several generations given birth to males who were all affected with hypospadias; whilst Lecat's[2] statement that a degree of hypospadias is not uncommon among families in Normandy may, if correct, be referred to the occasional combination of hereditary with endemic influence. In addition to such cases, there are other congenital anomalies of the urethra in which a corresponding exactness of repetition has been observed; as, for example, in the somewhat opposite condition in which duplication of the urethra occurs. A case of this description has been recorded by M. Picardat,[3] in which a father and son both had two urethræ, the orifices of which were situated one above the other; the one, it is said, serving for the discharge of urine, and the other for the passage of semen.

The same hereditary restriction of a defect in development may not unfrequently be observed in cases of congenital peculiarities and defects of the eye, however trivial the defect itself may be; as in a case which has been lately recorded, "in which the two eyes are of different colours in an entire family of cats."[4] With regard to this case, it may be remarked that, although as a rule congenital peculiarities in colour cannot be expected to illustrate any restrictive influence in reproductive development so well as congenital peculiarities of structure, yet there are some cases of this description in which the recurrence of the peculiarity appears to be associated with some amount of limitation by age. It is chiefly, however, in cases which are characterised by an absence of colour that the influence of this hereditary restriction can be either traced or reasonably assumed; for in such cases it may be inferred that if the pigment had been present, the deposit of it would have commenced at a definite period of development. Strictly speaking, the distinction which some have sought to establish between the inheritance of colour apart from or independent of structure is on all occasions more apparent than real, since it is probable that every transmitted peculiarity connected with either the presence or the absence of colour is associated with some transmitted peculiarity of structure; and if so, it would follow that an hereditary difference of colour in the two eyes is as much dependent on some abnormal change of structure in these parts as is the deficiency of pigment which is characteristic of albinoism; a defect which is now very generally acknowledged to be the result of an arrest of development. In many of these cases of deficiency of pigment, the recurrence of the defect is a family peculiarity limited to the offspring of the same womb; and the hereditary influence in such cases may in consequence be assisted to some extent by the reaction of the first albino fœtus on the maternal system; as in a case recorded by Dr. John Davy,[1] of four children in the same family, all of whom were albinoes; and in another case mentioned by the same observer, in which there were three albino sisters in a family of seven children. There have, however, been a few cases of albinoism observed in which no such difficulty in tracing the hereditary transmission of the defect could occur, owing to the fact of its being directly traceable to the male parent, although it has been somewhat erroneously asserted that male albinoes are not as a rule fertile. Prichard,[2] for example, has mentioned a case in which there was atavic transmission of albinoism through the male line in the negro race, the grandfather and the grandchild being albinoes, whilst the intermediate man was black. 'The Naturalist' for August, 1838, quotes (from 'The Journal of the Asiatic Society of Bengal') the case of a Malay lad, an albino, who had an albino sister; the parents were of the usual colour, but the grandfather (not stated which) was an albino; and M. Coinde[3] has lately submitted a case to the French Academy of Medicine, in which a man "had successively, by two different wives, three albino children." It was specially noted by M. Coinde, in this case, that there was no appearance of albinoism in the father of these albino children, but that he had become in some measure cretinized by the immoderate use of brandy. Whether, as is suggested, the case is in any way capable of showing how albinoism may be artificially produced, is doubtful; but there can be no doubt that the occurrence of the defect was, to say the least, associated with hereditary influence, in the same way that albinoism—or what may, perhaps, in some cases be more correctly described as an abnormal whiteness of the hair and skin without the peculiar appearance of the eye which Dr. Bostock[4] refers to as "a strictly hereditary" condition—apparently dependent in the first instance on endemic

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[1] Quoted by Dr. James Y. Simpson, 'Cycl. Anat. and Phys.,' vol. ii, p. 735.

[2] Ibid.

[3] 'Recherches sur les Anomalies congénitales de l'Urètre;' Thèse de Paris, 1858, No. 91.

[4] 'British Quarterly Review,' art. "Physical and Moral Heritage," January, 1859, p. 10.

[1] 'Psychological Researches,' pp. 395, 396; 1863.

[2] 'Researches into the Physical History of Mankind,' 4th Edit., vol. i, p. 368 1841.

[3] 'Comptes Rendus,' tom. lii, pp. 214, 215; 1861.

[4] 'Cyclop. Anat. and Phys.,' art. "Albino," vol. i, p. 86.

influence, often shows a tendency to recur as a family defect. There are many cases of this description on record, in which albinoism or abnormal whiteness has apparently been due in the first instance to some local influence, and has recurred in certain families in consequence of the local being afterwards associated with hereditary influence. Dr. Livingstone,[1] for example, in his travels in Central Africa, observed in two small hamlets near Kariba, on the Zambesi, that "many" of the inhabitants were albinoes; and close by there was a white hippopotamus in a herd, which "seemed to be the father of a number of others, for there were many marked with large white patches." The effect of this combination of endemic with hereditary influence in the suppression of colour is also very characteristically shown in the celebrated race of white cats, for which the district around the Lake of Van, in Armenia, has long been famous.[2] These cats, which are much larger than the common cat, abound in this particular district, where no other kind is to be met with; and they are noticeable not only for the snow-white colour of their fur, but also for their long and bushy tails. This peculiarity of the tail, which is also observable in the Persian race of cats, cannot perhaps be referred to one period in development more than another; but there are other cases in which structural peculiarities of the tail in these animals, sometimes unassociated with any transmitted peculiarity of colour, may be admitted as illustrations of the restrictive influence of age; such, for instance, as the abnormal development which leads to the curve or bend in the tails of Malay cats, and the arrest of development which reduces the tail of the Manilla and the Manx cat to a stump: in the latter case there is an hereditary increase in length of the hinder legs, which is suggestive of the kangaroo type.[3]

In all or nearly all of the preceding cases of congenital defects, the influence of age may reasonably be inferred to have restricted the development of the peculiarity to some definite period of fœtal existence; the positive evidence in favour of this inference being derived from the exactness with which each repetition of the family peculiarity recurred, whilst the negative evidence, which is equally conclusive in its favour, was based on the complete exemption of some members of the family from a peculiarity which, when it occurred at all, always presented the same form. Moreover, with respect to these occasional exemptions from a family defect, it has been already remarked that no very satisfactory explanation can at present be offered; for although, when the time for any particular occurrence of abnormal development has passed away, there is usually an absolute freedom from it, yet it not unfrequently happens that in the descendants of those who have been thus personally exempt there is a recurrence of the peculiarity, which, as it exhibits an exact repetition of the same abnormal form, can only be referred to the same period of development.

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[1] 'Narrative of an Expedition to the Zambesi and its Tributaries,' 1864-5, p. 326.

[2] 'A Journey from London to Persepolis,' by John Usher, F.R.G.S., p. 338; 1865.

[3] 'On the Peculiar Formation of the Tails in the Malay and Manilla Cats,' by George Bennett, M.R.C.S. &c., 'Medical Gazette,' vol. viii, 1831.

Even when, instead of an exact repetition or a complete absence of a family defect, there is a slight but well-defined trace of it to be seen in the part previously fixed upon for its hereditary occurrence, the evidence in favour of the restrictive influence of age is quite as strong as in the preceding cases, whether the trace on such occasions be regarded as cicatricial or not. For whatever difference of opinion may be expressed with respect to the nature of the pathological change which takes place in these cases, there can be no doubt that there is usually, if not always, strict limitation in the time of its occurrence, since the laws of embryonic development only admit the possibility of the defect occurring within a very circumscribed period of fœtal existence. This is specially noticeable where the traces of abnormal development refer to fissures which are normally closed at a very early date; as, for example, in cases of hereditary hare-lip, in which the defect is due to a local arrest of development occurring not later than the second month of intra-uterine life, and which in a case mentioned lately by Sir William Fergusson[1] was propagated through five generations; whilst in other cases it has shown itself in several members of the same family without having affected the parents, as in a case recorded by Mr. James Lucas,[2] in which four out of five children were born with hare-lips. With reference to cases of this description, M. Demarquay[3] states that "it has sometimes been noticed that the children of parents attacked with this vice of conformation are born with an upper lip constricted, as if it had undergone a slow reunion, in every respect analogous to that which is obtained after the operation for hare-lip;" and he seems inclined to admit with Rennes, Dieudonné, Bitot, Marjolin, and Desormeaux, that the appearance of the cicatrix in these cases indicates that "hare-lip may be cured in the mother's womb." A reparative process of this kind would not perhaps be more difficult to accomplish than those intra-uterine operations by which a limb or part of a limb is supposed to be more or less dexterously removed by a ligature formed of the umbilical cord. But it is questionable whether such an explanation of the pathology of these cases is correct; for although there are other cases in which a corresponding trace of an hereditary defect has been observed, such, for example, as in some cases of hereditary coloboma iridis, in which the fissure which has been represented in some members of the family by a lineal scar might be supposed to have been caused by the spontaneous approximation of the two sides of a normally developed cleft iris—a supposition which has the same evidence to support it as the preceding class of cases, since fissure in the iris is originally a normal condition as it is in the lip—yet there are other cases in which no such process of reparation could have occurred; as, for example, in cases of imperfect development of the iris in a parent, followed by its complete absence in some of the children, whilst in others it has been well formed, showing that the defect in these cases merely represents different degrees of arrest in development. Dr. Heutzschel,[1] of Chemnitz in Saxony, has recorded a case of this kind in which a man with congenital absence of the upper portion of the iris had five children, in three of whom the iris was altogether absent, and in two it was well formed. There are, moreover, cases of an equally exceptional character, in which the presence of an abnormal trace referable to an hereditary defect could not, with reasonable probability, be explained by any self-healing process in a fully-developed defect founded on an application of this theory of an early closing movement in the affected parts. For when the trace of abnormal development refers to an hereditary excess instead of arrest of development—such, for example, as may occasionally be noticed in cases of hereditary double-thumb, in which the customary duplication of structure is replaced by an indented line which is almost imperceptible—it would evidently be wrong to regard it as an indication of a self-repaired defect; since it merely shows how, like some of the hereditary defects in the iris and the lip above referred to, the normal progress of development is liable to be disturbed at a certain period in the formation of these parts, and that, however slight the effect which may at this particular time and place be produced, there is a complete freedom from any further mischief. Consequently, it is found that, like the acquired habit in a horse of shying always at the same milestone and at no other on a long road, the difficulty in these cases has only to be encountered once; and therefore it is only allowable to suppose that the imperfection referred to in the iris and the lip may represent a short delay in development, showing that the recurring attempt to arrest development had been tried, and had not quite failed.

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[1] 'Lancet,' December 6th, 1862, p. 619.

[2] 'Memoirs of the Medical Society of London,' vol. iv, p. 100, 1795.

[3] 'Nouv. Dict. de Méd. et de Chir. Pract.,' art. "Bec-de-lièvre," p. 669, tom. iv, 1866.

In many forms of hereditary disease, and especially in those affecting the brain and nervous system, it is in some respects obvious that the chief difference between a congenital and a non-congenital defect, such as amaurosis occurring before or after birth, is often one of degree as regards the extent of the malformation rather than of time as regards the date of its occurrence. When the malformation itself, beginning at the same period of fœtal development in two or more members of the same family, or even the simple inability to see, is recognisable at or soon after birth—such as occurred in Dr. Newman's[1] case of two sisters who were born blind in consequence of the "absence of the optic disc and retinal vessels;" in Mr. Hutchinson's[2] case, also of two sisters born blind, in consequence of congenital amaurosis with white atrophy; in Dr. Monteath's[3] case of the eldest three children in a family at Paisley, who were born amaurotic from some structural defect which is not specified by the narrator of the case; in Mr. Lyall's[4] case of congenital amaurosis affecting three children, two boys and a girl, in a family of seven; and in a corresponding case which has been recorded in connection with it, in which three children in a family of high rank were similarly affected, whilst five other children in the same family were exempt, in all of which cases the inability to see, if not the structural defect itself, could be readily diagnosed,—there can be no difficulty in recognising the restrictive influence of age in abnormal development during fœtal life. But when the occurrence of blindness from amaurosis has been observed at a later period of life, and has been limited, as is frequently the case, to the same age in different members of one family; or when, in consequence of the limitation by age being associated with atavism, the relationship is less close—a well-marked illustration of which occurred in the practice of the late Dr. Robertson,[5] in which "a grandmother and four of her grandchildren (sex not stated) were affected with amaurosis, and in all of them the complaint appeared about the age of puberty,"—it might be somewhat hastily assumed to be independent of any peculiarity in the development of these structures during fœtal existence. A distinction like this is usually, perhaps, more apparent than real; for if our powers of observation were such as to enable us to trace diseases to their source, it would probably be found that the restrictive influence of age which is shown in the parallel evolution of a disease or defect in conjunction with a certain period of development in the air-breathing life, would, generally speaking, have to be referred to a much earlier date. This is sometimes very distinctly shown in the parallel development, both normal and abnormal, of twins belonging more especially to the same sex; as in the cases recorded by Mr. Lawrence,[1] M. Boudin,[2] Dr. Underwood,[3] Camper,[4] and Dr. Lever.[5] The same parallel evolution is often observed in the morbid development of twins after birth; as in the cases related by Dr. Elliotson[6] and M. Trousseau;[7] in the case of the twin-brothers Laustand;[8] in a case cited by Dr. Laycock,[9] of "morbid sympathy between twin brothers," which was communicated in 1834 to the French Academy by M. Cagentre; in some other cases of a similar description which have been noticed by writers on mesmerism, who have attributed the singular coincidences observed to animal magnetism;[10] and in M. Baumr's[11] case of suicidal development in twin brothers. It may be useful to remark, that the attempt at suicide which failed as regards one of the twins in this case has been fully carried into effect in other cases, as in the twin brothers referred to on a former occasion,[12] who, although living at the time in different parts of the country and without communication, yet committed suicide at the same age.

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[1] 'Ammon. Zeitschr. f. Ophthalmologie,' quoted in the 'Lancet,' 1830-1, vol. i, p. 384.

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[1] 'Ophthalmic Hospital Reports,' vol. iv, pp. 202-4, 1864.

[2] 'Med. Times and Gaz.,' December 12th, 1863, p. 617.

[3] 'A Manual of the Diseases of the Human Eye,' by Dr. Charles H. Weller, translated by George C. Monteath, M.D., Glasgow, vol. ii, note, p. 82, 1831.

[4] 'Edinb. Med. and Surg. Journal,' vol. xiii, p. 132, 1817.

[5] Ibid., vol. xxxii, p. 292, 1829.

When, therefore, the appearance of an hereditary disease has been limited to a particular age after birth, it may reasonably be inferred that a certain amount of time, dating back in many cases to some period of fœtal life, would often be required to accomplish the abnormal changes in development which, like those preceding the decay and fall of the leaves of a tree, are ultimately disclosed to our notice by the occurrence of disease and death at a fixed age. Even in those cases in which there is an hereditary prolongation of life beyond the usual term, together with an inherited freedom from disease, the same necessity for dating back the commencement of this restraining influence in development may also prevail; and it is interesting to notice that the subject admits of being in like manner illustrated by analogies derived from the same source. For not only may the classic allusion to the fall of the leaf be accepted as generally applicable to the "race of man," but there are also hereditary exceptions to this leaf-falling occurrence which are quite as, if not more, strongly marked than the exceptional inheritance of a green old age. For cases have been observed in which, whilst the trees around them have shed their leaves at the appointed time, others of the same species have remained evergreen, in consequence of some peculiarity of structure which appears to have been transmitted as an heritage for many succeeding generations.[1]

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[1] 'A Treatise on the Diseases of the Eye,' 3rd Edit., p. 534, 1844.

[2] 'Archiv. Gén. de Méd.,' Fév., 1865.

[3] 'Diseases of Children,' Tenth Edit., p. 541, 1846.

[4] Cited by Bouchut, 'Practical Treatise on the Diseases of Children and Infants at the Breast,' transl. by P. Hinckes Bird, F.R.C.S., p. 149, 1855.

[5] 'Medical Gazette,' vol. xxxviii, p. 946, 1846.

[6] 'Lancet,' 1830-31, vol. i, pp. 770-1.

[7] 'Clinique Médicale,' 2nd Edit., tom. ii, p. 385, 1865.

[8] 'Brit. and For. Med.-Chir. Review,' July, 1844, p. 109.'

[9] 'Lancet,' 1842-3, vol. i, p. 425.

[10] 'Brit. and For. Med.-Chir. Review,' vol. xviii, p. 169, 1844.

[11] 'Annales Médico-Psycholog.,' 1863, No. 2.

[12] 'Med.-Chir. Review,' April, 1863, p. 529.

(*To be continued.*)

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[1] A well-authenticated instance of this hereditary persistence of what appears to be an acquired peculiarity occurs in the case of the evergreen plane-tree of the island of Crete. The existence of an "extraordinary evergreen plane-tree" in Crete is mentioned by Pliny, who states that it grew on the banks of the Lethæus, and that attempts were made to propagate it throughout the island; but that it degenerated, shedding its leaves in autumn, as all other plane-trees. It may be remarked that such artificial attempts to propagate the tree might be expected to fail, in consequence of variations in the endemic influence; which having, it may be inferred, originated the variety, had become associated with, and necessary for, its reproduction. But when the conditions favorable to the maintenance of this acquired peculiarity have been essentially the same, the plane-tree has continued throughout the year to retain its leaves. It is to some extent uncertain, however, whether the evergreen plane-tree has without intermission flourished in Crete since the time of Pliny, or has only appeared in interrupted or atavic succession. Other writers in early times besides Pliny have mentioned its existence; but when Tournefort, the well-known botanist, visited the island in the early part of the last century, he sought for it diligently, yet without success; the inhabitants having retained no tradition of the tree. The interesting fact that an evergreen plane-tree still grows in Crete has been recently made known to us by Captain Spratt, who had the opportunity of seeing it on an estate belonging to an English merchant, and situated near the village of Latraki, in a valley nine or ten miles from the town of Kania. "Here (writes Captain Spratt, 'Travels and Researches in Crete,' vol. ii, pp. 40—2, 1865) we were taking a walk together, when he said, 'Now I will show you a curious plane-tree that never loses its leaves;' and taking me to a steep part of the valley to the north of his house, he pointed out two rather young and branching plane-trees that grew from the side of the rivulet glowing in the ravine; they were surrounded by many others, but these two alone retain their leaves during winter . . . . . These two trees were suckers or saplings from the roots of a very large one of the same kind, which he had cut down, after purchasing the property, for some requirements, not knowing its peculiarity." The only noticeable variation in structure was that the leaves of the evergreen trees were thicker and less flexible than those of other plane-trees.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

Member of the Royal College of Physicians, Physician to the St. Pancras and Northern Dispensary, and to the Eastern Dispensary, London.

On the Medicinal Uses of the Ptelea Trifoliata. By Dr. O. F. Potter, St. Louis, United States.—Dr. Potter has been familiar with the medical virtues of the *Ptelea trifoliata* for some years, and recommends it to the favorable notice of the profession. It is known in America as the wafer-ash, or wing-seed, a species of swamp dogwood, and belongs to the natural order of Xanthoxylaceæ. It is a shrub from six to eight feet high, with trifoliate leaves (whence its specific name) and a winged fruit. It grows abundantly west of the Alleghanies, in shady, moist, and rocky places. The bark of the root possesses the peculiar medicinal properties, which it yields to boiling water, although alcohol is the best solvent. Dr. Potter has used this bark as a tonic to follow the use of quinine in all grades of fevers, and also in cases of general debility connected with gastro-enteric irritation. It is mild and unirritating, having a soothing influence on the stomach and promoting digestion. It improves the appetite, enabling the stomach to bear suitable nourishment, and is said to be tolerated when almost all other tonic or stimulant remedies are rejected. Dr. Potter has found it specially useful in cases of debility following a low grade of fevers, in improving the menstrual functions in females after confinement, and in debility connected with scrofulous sores. He administers it in the form of tincture of the bark, and the dose is from one to two tablespoonfuls three times a day. It appears that it has long been used by the negroes of the South, who call it the scrofula root; and the old French inhabitants near St. Louis used it many years ago as a cure for intermittent fevers, long before quinine was known.—*New York Medical Journal*, December, 1865.

On the Use of Chloride of Iron in some Affections of the Skin. By Dr. Bedford Brown, of Washington City.—As the chloride of iron combines active tonic and alterative virtues with a remarkable power of controlling certain local inflammations, probably due to very active astringent or hæmostatic properties, it seems to be well adapted to the treatment of several affections of the skin. Dr. Bedford Brown has used it extensively in this class of diseases, and he thinks it equally applicable to the acute or chronic forms. The hæmostatic power of chloride of iron consists in a peculiar action on both vessels and fluids, tending to diminish the capillary circulation and to arrest hæmorrhage; and this principle of action may be applied to the control or regulation of that part of the circulation under circumstances of dilatation, engorgement, or inflammation. It is incontestable that the chloride of iron has the power of arresting or diminishing the secretion or excretion of pus, or the exudation of lymph, and thus it influences these processes as resulting from inflammation. Dr. Bedford Brown considers that this preparation is serviceable in severe and malignant cases of smallpox, as it modifies and diminishes inflammatory action, and curtails the process of suppuration; and he thinks that it even possesses considerable abortive powers over the disease. In chronic pustular and vesicular affections of the skin Dr. Bedford Brown believes that the chloride of iron possesses great efficacy, not only in removing the immediate local affection, but in correcting the predisposing causes. In carbuncle and phagedænic affections of the skin it seems to arrest the progress of ulceration, and causes granulations to take the place of sloughs. In phagedæna occurring during the progress of syphilis, when mercury cannot be employed, Dr. Bedford Brown has found the combined use of chloride of iron and chlorate of potash to be attended with very beneficial results.—*American Journal of the Medical Sciences*, April, 1866.

On the Application of Carbonic Acid in the Treatment of Diseases of the Urinary Passages.—M. Démarquay has lately written a work 'On Pneumatology,' in which he discusses the advantage of the exhibition of carbonic acid in diseases of the genito-urinary organs. This gas was employed in the last century chiefly as a solvent of calculi, but it is probable that the beneficial effects observed were due rather to the soda or potash employed in the process of extricating carbonic acid than to the carbonic acid itself. But a more useful and practical application of the medicinal properties of carbonic acid has been made in certain cases of cystitis and vesical neuralgia. Last summer M. Démarquay witnessed a case of vesical neuralgia occurring, without any apparent cause, in a woman aged thirty, and giving rise every day to numerous paroxysms of five or ten minutes' duration. Injections of carbonic acid repeated twice a day induced rapid improvement, so that after four days three paroxysms only occurred in the twenty-four hours, and a cure, but perhaps a temporary one, was effected in a fortnight. In such cases the carbonic acid acts as a special sedative. The vesical douche may be easily administered with a small caoutchouc bag filled with carbonic acid, which is propelled into the bladder through a common catheter. But another and more complicated system exists in the use of an apparatus, known as Mondollot's, and consisting of a double-barrelled tube, which facilitates the escape of any gas in excess from the bladder. The probable utility of carbonic acid as a sedative in some uterine affections characterised by local irritation might be inferred from its known physiological effects. Thus it has been successfully employed in the form of fumigations in amenorrhœa and dysmenorrhœa, and M. Démarquay thinks that in such cases water charged with carbonic acid might be a good substitute for the gas itself. Among the various uterine affections iu which M. Démarquay has derived benefit from the carbonic acid douche are chronic enlargements and simple ulceration of the cervix; and the same remedy has succeeded in allaying pain, in checking fetid secretions, and occasionally in healing ulcers. M. Démarquay, however, does not regard carbonic acid as a panacea in the above-mentioned complaints, but thinks it may be of considerable service in certain cases.—*Journal de Médecine et de Chirurgie Pratiques*, October, 1865.

On the Physiological Effects of Narceine, and its Therapeutical Action in certain Diseases of Children. By Dr. S. V. Laborde.—When narceine was first discovered in 1832, it was made the subject of experiment on animals, and was declared by Magendie to be inert; but since that time M. Ch. Lecomte, and still more recently M. Ch. Bernard, have ascertained that it possesses hypnotic properties. It is found, in fact, that there is a constant uniformity in its action upon the lower animals, which exhibit under its influence the same kind of sleep and the same mode of waking, and the physiological phenomena are always identical. Dr. Laborde has, therefore, studied the action of the alkaloid on the human subject, his present observations having been made on children. In the child, as in the adult, narceine, when properly administered, produces hypnotism; but the narcotic sleep has this special peculiarity—that it does not induce, after waking, any heaviness of head or painful sensations in the digestive canal, or tendency to fainting, such as are commonly known to follow the administration of the other alkaloids of opium, especially codeine and morphine. The mode of administration recommended by Dr. Laborde is 25 centigrammes (about 3½ grains) to 500 grammes (a gramme is about 15 grains) of simple syrup, with a sufficient quantity of acetic acid. In this formula each tablespoonful containing 20 grammes of syrup contains one centigramme of narceine. This preparation was administered to children suffering under various complaints attended with cough and irritation, and was almost always followed by beneficial effects, and its hypnotic and sedative properties appeared likely to be attended with good results in whooping-cough. It was accordingly given in two cases of this disease, in one of which the action was negative, owing to an attack of measles, but in the other was quite satisfactory. Dr. Laborde admits that in the two cases of whooping-cough it might be alleged that the disease was in a stage when it was more amenable to treatment than at first, but still he does not doubt the reality of the influence of the narceine.—*Bulletin Général de Thérapeutique*, September 15th, 1865.

On the Employment of Narceine in Medicine. By Dr. A. Eulenburg.—The sleep produced by narceine differs from that of morphia by its tranquillity and the absence of snoring, as well as by the waking which follows its use: neither paralysis nor disturbance of the intellect is observed such as always follows the torpor of morphia, and animals return much more quickly to their normal state than after the use of morphia. The publication of the above views by M. Bernard induced M. Eulenburg to make frequent use of narceine, especially in surgical cases, and even on persons in health. It may be used either internally or externally. The last method, by subcutaneous injection, has been proved to be preferable by the certainty of the effects produced and by the smallness of the doses required. M. Eulenburg employs for this purpose a solution of hydrochlorate of narceine, containing from the eighth to the fourth of a grain; but for internal use he employs from the sixth to the half of a grain. In healthy persons these doses have been generally followed by a slight narcotic effect, without headache or gastric derangement. Among the physiological effects of narceine, in addition to narcotism, is its action on the circulation, consisting in a diminution of the pulse, succeeded by acceleration. M. Eulenburg considers it superior to all other substances in its sedative and hypnotic effects, and that its employment is indicated in articular affections, phlegmons, lesions of the eye (as iritis), orchitis, cystitis, and after painful operations. In all such cases, when given in the above-mentioned doses, either internally or externally, it rapidly relieves pain and produces a gentle sleep. Among the cases published by M. Eulenburg there were several in which morphia produced only a negative or insufficient result, in consequence of want of tolerance or of some other cause, and in which narceine acted in a most satisfactory manner. He has hitherto had but few opportunities of employing narceine in neuralgia; but when he has done so, rapid improvement has followed its use.—*Bulletin Général de Thérapeutique*, May 30th, 1866.

On the Therapeutical Use of the Phallus Impudicus in certain Diseases. By Dr. Kaleniezenko.—This fungus, which grows in large quantities in the Ukraine and the neighbourhood of the Caucasus, and is completely developed in the months from June till August, is employed by the inhabitants of those districts pretty frequently as a medicine. The parts used are the sporules, which are found under the fallen leaves of trees, and are of a white and velvety appearance. They contain a large quantity of thick, glutinous, albuminous-looking mucus, in the midst of which is the embryo. As the latter grows, the mucous fluid diminishes, and becomes thick, and assumes the well-known fetid smell. The people of the country collect the mucous fluid in bottles, and pour melted butter over it to keep out the air; and by this means it can be kept unspoiled for a whole winter. It is applied for rubbing into painful limbs. The plant itself is dried in an oven or in the sun, is kept well stoppered, and is used only internally. The infusion of the powder, having the smell of the plant, is given in chronic rheumatism, in the proportion of an ounce to three quarters of a litre of water or half a litre of brandy, three half-tablespoonfuls being given during the day. The treatment is said to last no more than eighteen days; and the medicine causes sweating, loss of appetite, thirst, and perspiration. The phallus is also given in acute and chronic gout, in dropsy after intermittents, and in nocturnal pains of the bones. Gouty complaints are principally treated by rubbing the joints near a fire or in the sun, together with the internal use of the infusion, and pains in the bones are treated in a somewhat similar manner. The external application is said to cause a scarlet eruption on the skin.—*Schmidt's Jahrbücher*, July, 1865.

On the Arsenical Treatment of Fevers. By Henry C. Brodrick, M.D.—Dr. Brodrick commenced the treatment of periodical fevers in India by large doses of arsenious acid in the year 1863; and he has kept notes of 177 cases of fever thus treated, most of the cases being of the quotidian type. The large quantities administered to the patients may be estimated by the fact that the entire amount taken by the 177 cases was more than 10½ ounces of the liquor arsenicalis, the largest quantity taken by one case being rather more than two ounces, and on an average each case took four drachms and forty-five minims during treatment. All the cases yielded to the use of the drug, quinine being given to none of them, and very few took emetics or purgatives. The usual dose taken by each patient was half a drachm of the liquor arsenicalis, three times a day, immediately after meals; but Dr. Brodrick has given it in as large a quantity as a drachm and a half of Fowler's solution for one dose. He also states that he has taken the remedy himself, being cured of a quotidian by six half-drachm doses of the arsenical solution; and he adds that although these doses did not produce vomiting, they made him feel very sick and miserable. All the cases in which the arsenic was given were cured, each case taking seven days on an average. Dr. Brodrick does not assert that arsenic is superior to quinine in the treatment of periodic fevers; but he states that if it is equal to it, and that it is the proper medicine for charitable dispensaries, considering the high price of quinine in India, the temptation to the poor patient to hoard and sell it, and the fact that arsenious acid may be procured in every bazaar in India at a very trifling cost.—*The Madras Quarterly Journal of Medical Science*, August, 1865.

On the Use of Digitalis in the Treatment of Mania, recent and chronic. By S. W. D. Williams, M.D.—This paper, together with nineteen illustrative cases, is written in confirmation of the opinion of Dr. Robertson as to the efficacy of digitalis in certain forms of mania. The cases already published by Dr. Robertson tend to show the almost specific action of digitalis in allaying the excitement of impending general paresis; but the paper of Dr. Williams is confined to illustrating the use of the drug in the excitement accompanying mania in its acute and chronic forms, and also when complicated with epilepsy. The results of the cases show that digitalis seems to possess a marked power of arresting cerebral excitement, of whatever nature it may be; but it is also evident that, except as allaying excitement, it has no further curative powers. Dr. Williams considers its efficacy in chronic mania and epilepsy to be simply due to the fact that it lessens the action of the heart, thus diminishing the flow of blood to the brain, and offering less food for the excitement to feed on. This view is rendered more probable from the fact that digitalis never exerts any beneficial influence until after the pulse has been affected; and Dr. Williams has moreover observed that when the system has become used to the potency of the drug, the return of the pulse to its former rate is accompanied by a corresponding exacerbation of excitement, which can only be allayed by increasing the dose. Although it might be supposed that only strong, healthy constitutions would be able to bear the effects of digitalis, this view is incorrect, for patients weakened by disease or exhausted by excitement bear its administration in general better than those who are stronger; and this fact explains its efficacy in general paresis, which is essentially a disease of debility. Dr. Robertson advises the use of doses varying from ʒss to ʒj three or four times a day, under which treatment an excited patient may perhaps at first become more excited than before; but if the medicine is continued, it will be found that the excitement gradually subsides, and the pulse becomes intermittent. When this is the case, the digitalis should be omitted until the pulse has resumed its normal rhythm. As regards epileptics, Dr. Williams thinks that digitalis possesses a certain amount of preventive power, and is able not only to ward off the attacks of violence, but to lessen their force when they occur. One of the patients whose case is recorded by Dr. Williams had taken ʒss of digitalis (tincture) twice daily for many months and with marked benefit, for one of the epileptic relapses passed off without the slightest manifestation of violence. In some cases where digitalis has after a time lost its power, or has caused sickness and vomiting, Dr. Robertson combines it with morphia, tincture of hyoscyamus, and chloric ether. The influence of digitalis on the heart being generally admitted, the question is whether it is a stimulant or depressant; and Dr. Williams inclines to the belief that it is a decided stimulant. The general conclusions drawn by Dr. Williams as to the use of digitalis in insanity are that it is a valuable sedative both in recent and chronic mania, and when these forms of disease are complicated with general paresis and with epilepsy; that the average dose of the tincture is ʒss to ʒj, to be continued even for many months, unless it causes intermittence of the pulse, when it must be immediately discontinued; that weakness of the circulation is no indication against its employment, but the reverse; and that in certain cases it may be advantageously combined with chloric ether, morphia, and prussic acid.—*The Journal of Mental Science*, January, 1866.

On the Febrifuge Properties of the Thevetia Nerifolia. By J. Shortt, M.D.—The *Thevetia nerifolia* is a tree which is common in the gardens of Southern India, and is said to have been brought originally from Nepaul, under the name of the yellow oleander. It is also sometimes called the "exile tree," from its solitary habit of growth. It belongs to the natural order of Apocynaceæ, and abounds in a white, milky juice. It possesses very active properties, being allied in its botanical characters to the *Cerbera Tanghin*, or ordeal poison tree of Madagascar. The tincture prepared from the bark of the *Thevetia nerifolia*, when taken in large doses, produces violent vomiting and purging, followed by collapse; in half-drachm doses it produces nausea and giddiness, sometimes vomiting and purging: but its effects seem to be irregular, vomiting being produced in some cases, diarrhœa in others, and in some cases there are both vomiting and diarrhœa. In ten-minim doses it had no effect unless it was repeated every second hour, when it either purged or vomited the patient. It seems to act like other vegetable acrids, producing violent effects on the stomach and bowels, and causing great prostration. Dr. Shortt relates the particulars of three cases in which the medicine was administered for the cure of intermittent fever, and in all of them the result was favorable; and he gives the abstract of twenty-eight other cases, in which the effects were of an uncertain nature, producing vomiting or purging in some; in others, nausea, headache, and tightness in the throat, and other unpleasant symptoms; and in some it produced no visible effect whatever: but in all it completely removed the fever for which it was administered.—*The Madras Quarterly Journal of Medical Science*, August, 1865.

On the Therapeutical Applications of the Permanganate of Potash. By Dr. Cosmao-Dumenez, of Finisterre.—Dr. Dumenez remarks, in the first place, that disinfectants act in three different ways; namely, by preventing the formation of fetid gases, by absorbing them, or by decomposing them; and as permanganate of potash acts in the last-named manner, it holds the first rank, and it is the more valuable as it is itself insipid and inodorous. This substance may be used either in solution or in powder mixed with carbonate of lime or starch, but Dr. Dumenez prefers the solution. He has treated many cases of ulcerated legs, exhaling a fetid smell and presenting a grayish appearance, and two or three dressings with the permanganate were sufficient to remove entirely the bad smell and to restore a roseate colour to the affected tissues. The permanganate, therefore, promotes the cicatrisation of wounds, and Dr. Dumenez has often seen this effect to follow from its use. The odour from gangrenous wounds disappears with surprising rapidity under the use of the permanganate; and even in cases of cancerous ulceration of the uterus, the use of injections of this salt improves the general state of the patients by putting them into a better hygienic condition. Ulcerated cancer in other regions may be treated with similar success by lotions or injections. In ozæna and in fetor of the breath the permanganate has been found very useful, and Dr. Dumenez quotes from Dr. Oliffe a case of ozæna thus successfully treated. In the case of morbid secretions it appears to remove from these liquids their septic character, and thus to improve the condition of wounds from which they may emanate. It never produces any bad effect, and when applied on mucous membranes, whatever the dose may be, it produces no pain or irritation. Dr. Dumenez passes in review the various substances which have from time to time been employed as disinfectants, such as acids, chlorine and the chlorides, pyrogenic oils like that of coal-tar, &c., and he gives the preference to the permanganate of potash over them all.—*Bulletin Général de Thérapeutique,* Nov. 30th, 1865.

On Training or Forced Exercise in the Treatment of Diabetes. By Professor Bouchardat.—Dr. Bouchardat, while admitting the efficacy of alimentary treatment in diabetes, considers it only as palliative, and he recommends the adoption of energetic exercise. This idea is not a novelty on his part, as in former writings he recommended, in the case of patients affected with this complaint, the energetic action of their bodies and arms; and then he ascertained that labour in the open air always promotes the utilisation of the feculent matters in diabetic patients. It is not sufficient in all cases to cause the disappearance of the sugar, but, all things being equal, in regard to the quantity of feculent matters absorbed and other conditions, a diminution in the proportion of sugar contained in the urine always coincided with exercise in the open air. M. Bouchardat gives an instance of remarkable success in the treatment of diabetes attained by this treatment, the diet being carefully regulated and the urine being examined at intervals. Although the patient may at first be very weak, the adoption of exercise will gradually give him strength. It is of the greatest importance, according to M. Bouchardat, to use the strength in proportion as it returns; and daily exercise of the body, arms, and legs is indispensable. The greatest care must be taken to find some daily exercise which is agreeable to the patient—as, for instance, in the case of men, hunting, rowing, fencing, skating, billiards, cricket, &c.—or any ordinary manual employments, as sawing, cleaving wood, turning, and the active work of gardening; and in women, all the active household employments, especially those which require the action of the legs rather than standing without walking. Riding in a carriage is not to be adopted except when no other exercise is possible; but riding on horseback is a salutary kind of movement, although it cannot be substituted for all the others. Of all the modes of exercise, that which is most convenient must be chosen; and it ought to be energetic, so as to produce a thorough sweating over the whole body; and then all necessary precautions should be taken to prevent the chance of chilling the system. M. Bouchardat relates several cases in which his system was successfully adopted in the treatment of diabetes; he considers the exercise of the gymnasium especially useful when such an establishment is well conducted, and he gives some rules to be followed by the patients. When the exercise has been continued for about an hour, and all the body is bathed in sweat, the flannel should be changed, and the skin washed briskly with cloths soaked in cold water, then strongly rubbed with coarse gloves or towels, or flesh-brushes. Then the body is to be struck and kneaded, so as to produce a complete reaction, which is sustained by a walk of a quarter of an hour at least, the body being protected by good woollen clothes. The skin should not be neglected while these exercises are used, and salt-water baths, either warm or, what is better, cold, if they can be borne, are, according to M. Bouchardat, of almost invariable utility. During the treatment the diet must be carefully regulated, glycogenic substances being avoided while the urine is diabetic, and resumed only when the sugar has disappeared. The red wines of Bordeaux or Burgundy may be drunk; but sparkling wines, like champagne, should be avoided. Coffee and tea, without sugar, are sometimes suitable, but their employment must be regulated by the condition of the urine after they are taken.—*Bulletin Général de Thérapeutique*, Dec. 30th, 1865.

On a New Medicinal Substance called Jurubeba (Solanum paniculatum). By M. Stanislaus Martin.—The plants belonging to the order Solanaceæ are very seldom met with beyond the fifty-second degree of north latitude, but their varieties are so numerous in Brazil that this might be considered as their country; and they grow there in the herbaceous state, in the form of rather tall bushes or shrubs. Their active principle generally resides in the leaves and fruits, and in the bark of the stem or roots. A naturalist of Pernambuco has made some experiments with the *Solanum paniculatum*, called in in Brazil *jurubeba*. He has found it to possess such extraordinary properties, that he thinks it would be useful to introduce the plant into European practice; and already at Pernambuco a plaster, a powder, a syrup, a wine, a tincture, a watery and alcoholic extract, an electuary, and an oil are prepared with this substance, which is employed in all cases of intermittent fevers, liver complaints, splenic affections, catarrh of the bladder, anæmia, chlorosis, dropsy, and difficult menstruation; and it is said to be the most energetic tonic and deobstruent in the materia medica. The jurubeba will be presented to commerce in the form of leaves, fruits, and roots. The bark of the root and the leaves have a well-marked bitter taste. The root, when treated with water or alcohol, furnishes a bitter extract, which is more abundant in the cortical than in the woody part of the root.—*Bulletin Général de Thérapeutique*, January 15th, 1866.

On the Therapeutical Uses of Oxygen Gas. By Dr. Bricheteau.—After briefly alluding to the discovery of oxygen gas in the latter part of the last century, and the unsuccessful attempts afterwards made by Beddoes and others to introduce it as a therapeutical agent, the author describes the physiological phenomena caused by its inhalation, and he describes a form of apparatus devised for the purpose. This consists of an india-rubber bag, furnished with a tube and a mouthpiece, and a stopcock; and the patient takes a deep breath from the bag, which shrinks at each respiration, showing the quantity of the gas inhaled. In order to prevent the products of expiration from re-entering the apparatus, the lips should be closed and expiration should take place by the nose; or, what is better, the india-rubber tube should be compressed near the mouthpiece at the time of expiration. M. Bricheteau does not consider that oxygen is well adapted for phthisical cases except under special conditions, and he admits that in diseases accompanied by fever its effects may be injurious. But at the beginning of phthisis, in lymphatic and scrofulous persons, when the local symptoms are not well marked and there is emaciation accompanied by a persistent dyspepsia, thus favouring the formation of pulmonary tubercles, the inhalation of oxygen may be of service. By modifying the nutrition and supporting the organism, it may in certain cases improve the constitution and arrest the progress of the disease; it can do no more than this, but it is so far useful. The inhalations of the gas may therefore be usefully employed in phthisical cases attended with little cough, where there is emaciation and rapid loss of strength, and where there is want of appetite and faulty digestion. It has also been frequently used in the treatment of asthma, and it has produced an amelioration of the symptoms in some cases; but Dr. Bricheteau thinks that in anæmic diseases its good effects are incontestable. In surgical practice the local application of oxygen has been attended with favorable results in the treatment of senile gangrene.—*Bulletin Général de Thérapeutique*, February 28th, 1866.

On the Cure of a Case of Intermittent Supra-orbital Neuralgia by the Hypodermic Injection of Sulphate of Quina. By Dr. Bricheteau. —Those who object to the treatment of neuralgia by the subcutaneous injection of sulphate of quina conceive that it is impossible to inject a sufficient quantity of the salt to produce the desired effect, but the case recorded by Dr. Bricheteau seems to prove that this objection is unfounded. The patient was a man of forty-two years old, who had suffered previously from a long attack of tertian intermittent, but had at last been cured by quina. The fever did not return, but he was subsequently seized with neuralgic pains, for which he entered the Hospital Neckar, in Paris, and was treated by the sulphate of quina, and was cured in three weeks. Dr. Bricheteau's treatment consisted in injecting 50 centigrammes of the sulphate (a centigramme is ·1543 of a grain); and the formula consisted of 1 gramme of sulphate of quina, 10 grammes of distilled water, and 50 centigrammes of tartaric acid. He injected 5 grammes of this solution, choosing the anterior region of the left axilla, where the cellular tissue which covers the large pectoral muscle is abundant and lax. During the injection the patient complained only of a sensation of smarting. In the evening he said that his attack was less violent than usual, and instead of its lasting four hours it had lasted only two. On the next day Dr. Bricheteau repeated the process, but on the opposite side of the body, and injected 60 centigrammes of the sulphate, corresponding to 6 grammes of the solution described. Under its influence the attack lasted only an hour and a half, and the pains were less violent. In the evening the patient complained of singing in the ears, and on evaporating the urine it was found on examination to contain crystals of sulphate of quina. The injection was repeated on several subsequent days, and at last the pains ceased, the treatment having been continued for eleven days. Dr. Britcheteau thinks it important that the salt of quina should be dissolved in tartaric acid, as being less liable in this form to cause local irritation.—*Bulletin Général de Thérapeutique*, February 15th, 1866.

On the Treatment of Phagedæna by Chlorate of Potash. By Dr. E. Tillot.—Chlorate of potash has been employed for some years as an external application in ulcers, cancerous affections, and scorbutic gingivitis. It has been successfully used in the troublesome ulcerations following some blisters. It is daily used in the Hôpital des Enfans, in Paris, in the dressings of wounds covered with diphtheria. The chlorate may be used externally either in solution or as an ointment: in solution, in the proportion of 10 to 12 grammes (a gramme is about 15 grains) to 600 grammes of water; as an ointment, 2 grammes to 30 of lard; and it may be mixed with glycerine The mode of application differs according to the disease, and it may be by injection, lotion, or friction, once or twice a day. Dr. Tillot says that the chlorate has hitherto been very little employed in phagedæna, and as he has found it to produce a remarkable effect in this disease he thinks it interesting to communicate his experience of its effects. He relates six cases of phagedænic syphilitic disease, in most of which the chlorate was successfully employed; and he remarks that all the patients were free from constitutional cachexia, and were for the most part vigorous persons. In all of them the chancres were multiple, and belonged to the variety of *soft chancre.* The effects of the treatment were all manifested at the beginning of the medication; and although the contact of the chlorate was painful to bear, it was never so great as to compel its discontinuance. Its first effect, Dr. Tillot states, is to relieve the pains, when there are any; to diminish the intensity of the suppuration, and to modify its nature by changing the appearance of the morbid surface: but where it excels all other remedies is in its power of arresting the disease in its spreading character. The action of the salt in phagedæna is not rapid, but it is constant.—*Bulletin Général de Thérapeutique*, March 30th, 1866.

On Bromism. By Dr. L. Marcq.—The case described by Dr. Marcq is one of poisoning by bromine—an effect analogous to that sometimes caused by the use of iodine. The patient was a man about fifty years old, who was suffering under ulcerated laryngitis, but without pulmonary tubercles; and after many other remedies had been tried he was put under a course of bromide of potassium with cod-liver oil. A considerable improvement ensued, but as the patient wished to get well rapidly the action of the bromine was increased by the local application of bromine itself on the affected parts by means of a pulveriser. A week afterwards the patient had a dirty yellow complexion, hollow eyes, a strange fixed look, a face without expression, considerable emaciation, tottering limbs, trembling hands, and a generally cachectic state. Gradually the appetite was lost; intense pains supervened on the hairy scalp, especially at night; the strength was daily diminished, and the trembling increased in proportion. But as these symptoms of *bromism* developed themselves, the disease of the throat was relieved. As the fact of poisoning by bromine appeared to be established, it became necessary to eliminate the poison, and the bromine was therefore discontinued, and only emollients and light diet were prescribed. In two months the symptoms of bromism had declined, and eventually the patient recovered completely. He regained flesh and strength; the disease of the larynx appeared to be cured, and nothing was left of this affection except a slight sensation of pain on pressure over the left of the thyroid cartilage. The editor of the *Union Médicale*, while offering no opposition to the views advanced by Dr. Marcq, or to the treatment by bromine, remarks that a single case is not sufficient to establish either the theory or the practice, and that future experience must decide the questions thus proposed for the consideration of the profession.—*L'Union Médicale*, June 16th, 1866.

On the Application of Suphurous Acid Gas in the Cure of Contagious Diseases. By James Dewar, M.D., Kirkaldy.—In a pamphlet with the above title Dr. Dewar refers in the first place to experiments which he has instituted in relation to the therapeutical effects of sulphurous acid gas in the cattle plague, his apparatus consisting of a chafer two thirds full of red cinders, having in it a crucible and a piece of roll sulphur; and he states that this plan has been very efficacious as a prophylactic measure. But the success he met with in the treatment of cattle has been even exceeded, he believes, by the beneficial effects obtained by the same agent in the cure of various contagious and other diseases in the human subject. The cases in which the sulphur fumigations were employed were phthisical and diphtheritic patients, in all of whom there was a remarkable mitigation of the symptoms. Dr. A. Halliday Douglas, of Edinburgh, visited some of the cases, and adds his testimony as to the efficacy of the treatment. He states that he visited, with Dr. Dewar, three cases in which the patients had been subjected to sulphur fumigations for periods varying from three weeks to three months; in all three tubercular disease of the lungs had advanced to a considerable extent, and according to the accounts given by the patients themselves, their sufferings had been very materially relieved. Dr. Douglas had no doubt that the patients whom he saw had been the subjects of tubercular consumption, both the physical and general symptoms being well marked, and in one case there were unmistakeable signs of excavation. Dr. Douglas thinks that the results, though at present incomplete, are encouraging, and demand a further investigation of the subject; and he has caused a chamber for sulphur fumigation to be constructed in the Chalmers Hospital, where he intends immediately to test the powers of the remedy.—*Pamphlet.*

On the Therapeutical Use of the Waters of Vals (Ardèche). By Dr. Tourette.—The waters of Vals have been long distinguished by chemists and physicians for the variety of their composition and their medicinal efficacy. The springs are numerous, powerful, and copious; and their constituents, although essentially identical, vary in each spring, the quantity of ingredients differing very materially, and the strength of the waters consequently passing through a number of intermediate gradations. Thus in no place with which Dr. Tourette is acquainted are so many resources found as at Vals to vary the treatment of disease, and adapt it to all shades of temperament. He considers the waters of Vals superior to those of Vichy in diseases of the digestive passages, in which it is agreed that the treatment ought to begin with the least mineralised water of the springs, in order not to impose on weak stomachs a labour they cannot support; and Vals possesses a spring which is less mineralised by half than the least mineralised of the Vichy waters. These waters are also superior to those of the Allier in hepatic affections, in which waters are required containing bicarbonate of soda and free carbonic acid. They are said to be very efficacious in the treatment of uric gravel, and it is proved by experience that certain springs of Vals are superior to those of Vichy in the treatment of this affection. But the great superiority of the Vals waters over those of Vichy is in all affections caused by impoverished states of the blood, and under the influence of the ferro-manganic and ferro-arsenical waters of Vals many chlorotic, anæmic, and cachectic cases are restored to health. Dr. Tourette considers that all the diseases treated successfully at Vichy are treated with equal or greater success at Vals.—*Journal de Médecine et de Chirurgie Pratique,* June, 1866.

On the Employment of Digitalis in large doses in the Treatment of Pneumonia. By Dr. Gallard, of the Hôpital de la Pitié.—Among the numerous remedies which have been proposed and are now employed in the treatment of pneumonia, digitalis appears to be particularly indicated in cases where the febrile reaction seems to require the employment of antiphlogistics; but where the debility of the patient, and especially the state of depression into which he has fallen from the commencement of the disease would appear, on the other hand, to require the use of stimulants and tonics. These cases are not rare in practice, and they correspond pretty accurately to the form of pulmonary inflammation described under the name of *typhoid pneumonia*, and it is in such that Dr. Gallard has found digitalis most beneficial. He relates a case of a young man treated at the Hôpital de la Pitié with success by the use of digitalis. This drug, however, was not employed alone; but Dr. Gallard calls attention to the fact that the previous treatment by blistering and tartarized antimony produced no effect, and was discontinued because the patient was unable to bear it, owing to his weakness and depression. The digitalis was given in powders, each containing 5 centigrammes, to be taken every two hours. In twenty-four hours the pulse was reduced 20 beats, and in forty-eight hours 48 beats, having been originally 108, and being reduced to 60. The improvement in the local symptoms soon followed that of the general symptoms, and the patient, who was considered unable to resist an energetic antiphlogistic treatment, recovered in less than a week from a most alarming attack.—*Bulletin Général de Thérapeutique*, March 30th, 1866.

Cases of Chorea treated by Calabar Bean. By Dr. Ogle, Physician to St. George's Hospital.—The details are given of two cases treated by Calabar bean, at St. George's Hospital. The first was that of a young woman, an in-patient, aged twenty, on whom several remedies had been tried without effect, when Dr. Ogle directed her to take the Calabar bean, beginning with half a drachm of the tincture (having the strength of one drachm of the bean to one ounce of rectified spirit), administered three times a day, in water, and increased ten drops at a dose. She quickly improved in muscular steadiness, and was able to cut her own food, which she had previously been unable to do. The dose was raised to a drachm three times a day, and she eventually became quite well and left the hospital. The second case was that of a girl thirteen years old, an out-patient, for whom Dr. Ogle ordered twenty minims of the tincture of the bean, in water, three times a day. The dose was successively raised to thirty and forty minims, and in about six weeks she was discharged quite well. Dr. Ogle states that he has used the Calabar bean in other cases of chorea with marked benefit; but in some he found the results unsatisfactory. He considers that out-patients are better adapted for a trial of the bean than in-patients, because the latter are influenced by changes of food, and the quiet, and other beneficial conditions of the hospital.—*Medical Times and Gazette*, January, 1866.

On the Use of Atropia in Constipation and Stoppage of the Bowels. By Dr. Alexander Fleming, of Birmingham.—Dr. Fleming, when prescribing atropia for various diseases, frequently noticed that a slight relaxation of the bowels took place in from one to four days after the alkaloid was taken. If constipation had existed it was removed, and sometimes even purging was produced. Dr. Fleming believes that these results are due to increased peristaltic action of the bowels, perhaps caused by direct stimulation of the muscular coat by the atropia; but from the effect produced by atropia on other organs, he conceives that the mucous secretion of the bowels is checked, and their surface being thus unprotected, the irritation caused by the contents of the intestinal canal provokes more prompt and vigorous peristaltic action. But he also thinks that atropia, by constricting the smaller arteries, checks the supply of blood to the bowel, and thus relieves the congested muscle, and enables it to return to its normal condition. In cases of simple constipation, Dr. Fleming has employed atropia both in pill and in solution; but his later experience has led him to prefer its use in combination with some saline purgatives and tonics. He gives for a dose ten minims of a solution containing one grain of atropia to five drachms of distilled water, with the addition of a few drops of diluted hydrochloric acid, and sufficient rectified spirit to make ten drachms.—*British Medical Journal,* Dec., 1865.

On the Use of Phenic (Carbolic) Acid for the Cure of Phthisis. By Dr. J. R. Wolfe, Aberdeen.—Professor Longet lately made some experiments on himself with phenic acid for an attack of hæmoptysis and tuberculosis, and was so much relieved that the substance is now largely employed by several French physicians. Dr. Labort, of Vincennes, informed Dr. Wolfe that he had administered it to between 200 and 300 patients in different stages of phthisis, with marked benefit. The mode of administration was the following: Fifteen drops of the pure acid were dissolved in two drachms of spirit, and the solution mixed with thirty-two ounces of water, and this quantity was administered daily, partly by the stomach and partly by the inhalation of the fluid in a pulverised state, by some of the instruments now in use for reducing fluids to fine molecules. It is remarked by Dr. Wolfe that the internal administration of creosote and the inhalation of tar-vapour have long been recognised as useful remedies in phthisis; but the peculiar feature in the present recommendation in the use of phenic acid is its employment by the pulverising method, by which the larynx, trachea, and even the lung-tissue itself, are brought into direct contact with the remedial agent. As an adjunct to cod-liver oil, the phenic acid is beneficial in all stages of phthisis, but Dr. Wolfe considers it eminently useful in suppressing hæmorrhage from the lungs, in allaying irritation, and arresting profuse secretion.—*Medical Times and Gazette,* Nov. 25th, 1865.

HALF-YEARLY REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,

Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

The Occurrence of Pigment in the Blood and in Various Tissues and Organs in connection with Remittent Fever.—Dr. William Pepper has recently called the attention of the Pathological Society of Philadelphia to two fatal cases of remittent fever, in both of which he observed the existence of pigment masses in the blood, spleen, and other organs. The first case was that of a seafaring man aged twenty-seven. Blood drawn from the finger on the day before his death "showed about normal average of white corpuscles; red corpuscles pale, many of them crenated, with several large black pigment masses." The post-mortem, four hours after death, showed some congestion, but no ulceration of Peyer's patches and the solitary glands of the small intestine; no enlargement of the mesenteric glands; the liver was not enlarged; under the microscope the liver cells were seen to be of good shape, containing one or two distinct nuclei, and dark reddish granules, highly refracting, but irregular and angular in shape; there was also a great deal of floating granular matter and free masses of pigment. The bile from the gall-bladder contained brown yellow granular matter and pigment masses. Urine drawn from the bladder deposited granular casts, epithelial cells, fibrinous flakes and pigment masses. Spleen three times its normal size, soft, and, besides normal elements, contained reddish granular fibrinous flakes; large free pigment masses and cells (in size and granular nature very much resembling white corpuscles of blood) with pigment granules apparently in the nucleus; certainly the granules were in the cell-contents. Blood from portal vein, just above junction of splenic vein, contained many crenated corpuscles, a normal or slightly increased average of white corpuscles, and large black pigment masses. Blood from vessels on convexity of brain contained crenated corpuscles and hæmatine masses.

The second case was that also of a sailor who was brought into hospital, moribund from remittent fever, and died an hour and a half after admission. Blood obtained from the finger previous to death contained numerous white corpuscles, a deficient number of red corpuscles which were crenated and running together without forming rouleaux, and several black but rather small pigment masses. Post-mortem, four hours after death: pigment granules were found in every portion of the substance of the brain, in a few places large and free, but by far the greater portion were in the capillaries, at some places dotting their walls, at others collected into small plugs, filling the calibre of the vessel. Spleen was double the normal size, soft and dark. Its pulp contained, in addition to the usual elements, a great deal of granular pigment, either free or contained in the granular cells. In every portion of the liver large numbers of pig ment granules were found, either free or forming part of the contents of the hepatic cells. In the kidneys pigment was found both in the intertubular capillaries, in the capillaries of the Malpighian tufts, and also free in the form of large black angular granules. Small masses of black pigment were found in the urine contained in the bladder.—*American Journal of Medical Sciences*, April, 1866.

The Action of Fungi in the production of Disease.—Dr. Tilbury Fox points out that there are three lines of investigation having reference to the action of fungi in the production of disease, which may be represented by the three following queries: 1. Are the so-called parasites vegetable or not in nature? 2. What is the limit of variation in each kind of fungus? Are not most of the forms usually considered distinct, but varieties of a very few, perhaps one, species? 3. If vegetable, are fungi "accidentals" in disease, or do they necessarily directly or indirectly cause any special morbid changes; if so, of what nature are these? In answer to the first question, he asserts that the fungi are really of vegetable nature, that their germs are derived from the exterior, and that there is no such thing as a conversion of the elementary particles of animal structures into those of vegetable nature:—1. Because the fungi exhibit growth and independent life when removed from the presence and influence of all living animal structures; 2. Because no animal structure resists the action of liquor potassæ and other reagents in the same way that spores, sporules, and mycelia do; 3. Because identical parasitic forms are found in the hard structures of animals (*e. g.* bivalves, corals, foraminifera), and indeed vegetables, when no epithelial cell-structures of animal nature exist from whence the vegetable elements could spring; 4. Because no transitional forms are found, normal nuclei cannot be mistaken for sporules when the test of liquor potassæ is applied; 5. The fungus elements are first visible at the upper part of the hair-follicle, and migrate from above downwards towards the papilla and root. This is *à priori* evidence that the germs of the parasite are derived *ab externo*, and it is supported by clinical evidence and the results of treatment. On the question of the variation of fungi Dr. Fox states that observation is tending to overthrow the distinctions of species in most of the ordinary groups of cryptogams. He adduces several examples from those forms of fungi which do not infest the human body. With regard to human vegetable parasites, he sums up the results of the most recent observations:—"The parasitic fungi found upon the human surface are varieties which cluster around the penicillium or torula, in their sporular state as the type; that in the air the tendency is to the production of the aspergillus form of fructification: that the parasites of the mucous membranes, or entophytes, are more or less forms of oidium, mycelial forms of the sporous state of penicillium; that sarcina is a conjugated form of the latter. Chionypte Carteri, the fungus of the madura foot, being nothing more or less than a higher stage of the oidium, approaching closely the genus mucor. I do not comprehend the distinction of aspergillus, penicillium and mucor, as seen on the human surface, having noted all the appearances supposed to be characteristic of each in the development of the same fungus penicillium taken from the contents of the stomach of a pyrotic patient." The author notes seven modes of entrance of fungi into higher organisms:—1. That through natural orifices. 2. That in which the growing force forces the mycelial thread beneath the layers of the superficial tissues. 3. That in which processes shoot out from the spore and enter by such openings as stomata. 4. That where the cell-contents are absorbed. 5. That in which the spores are carried bodily inwards by growing parts; or, 6, dissolve away the opposing structures by chemical action; or, 7, enter by traumatic lesions. On the question whether the endosmotic action of the villi may not be able to account for the presence of cryptogams in the blood current, the author states that, as far as the ascertained facts of vegetable parasitism go, no proof of the possibility exists; there is probably no known instance of a growing plant in any situation not in direct or possible communication with the air. With regard to the part played by fungi in the production of disease, he points out that they may act *directly* (*a*) mechanically, *e. g.* in ordinary herpetic ringworm of the surface, where the mycelial threads range over the skin beneath the epidermis, and lead to erythema, &c.; or (*b*) chemically, they absorb oxygen and give out carbonic acid, and hereby secure to themselves the power of penetrating calcareous structures. In addition, a large amount of gas is evolved, as in cases of sarcinal disease. They also lead to fatty degeneration. In favus an examination of the old stubs shows that fatty changes are going on in the cell-structures. Or they may act *indirectly*, by bringing about changes in substances out of the body, which are brought to influence the latter; or by setting up a kind of fermentative action in part due to the oxidation consequent upon the nutritive changes in the plant; or by giving rise to products having an acute or a chronic action, and whose nature is at present a matter of doubt.—*Edin. Med. Journal*, April, 1866.

Thermometric Observations on the Fevers of Children.—Dr. G. Stevenson Smith, of the Children's Hospital, Edinburgh, has published some valuable thermometric observations on the fevers which attack children. He assumes the normal standard between the ages of three and ten to be about 99°. Three cases are recounted illustrating the value of the thermometer as a diagnostic agent in the febrile attacks of children—in neither of them did the thermometer indicate a higher temperature than 99°; and although in all there were well-marked febrile symptoms, the absence of increase in temperature indicated that no special fever was present. The result confirmed the accuracy of the conclusion. In ten cases of typhus fever the highest temperature registered was 104⅕°. The lowest temperature occurred during convalescence in three of these cases, when the heat of the body was 95°. In a fatal case of typhoid the temperature, two hours before death, was 104⅜°. In a case of ordinary scarlatina, with copious eruption and ulceration of the tonsils, the highest temperature registered was on the first day of the appearance of the eruption; it was then 104°. On the seventh day, when desquamation began, it was 97°. Dr. Smith observes that in only one of his typhus cases did a very sudden and great defervescence accompany the crisis. "In it the temperature, which, on the evening of December 25, was 101⅘°, fell on the following morning to 96⅘°, a fall of five degrees. But in most of the other cases the decline of the temperature to the normal standard, or under it, usually took place gradually, the different steps downwards scarcely ever measuring more than one or two degrees."—*Edin. Med. Journal*, March, 1866.

Intermitting Emprosthotonos caused by Cancer of the Brain.—Dr. Gemma relates the case of a woman, æt. 25, who had always enjoyed good health until the menses became arrested without any apparent cause. One day she was taken suddenly with severe pain in the forehead. She screamed out, and her jaws became fixed and her head carried forwards. She indicated by her gestures that she understood what was said, but that she could not open her mouth. The attack lasted three hours. Her symptoms then disappeared, with the exception of some confusion in her ideas, which, however, ceased the next day. The attacks returned every five or six days. In the intervals her health was perfect. She finally died during an attack. Autopsy: the arachnoid was injected; the left hemisphere of the brain was much softened, as was also, but in a less degree, the corresponding hemisphere of the cerebellum. The softening was more considerable in the anterior part of the left cerebral hemisphere than in the posterior. In the middle of this hemisphere was found a tumour the size of a pullet's egg, formed of a gelatinous matter, which enclosed a nucleus of the hardness of fibro-cartilage. The right hemispheres of the cerebrum and cerebellum, the corpus callosum and the medulla oblongata, presented no alteration.—*Gazetta Medica Italiana (Provincie Venete)*, and *Gazette Médicale de Paris, December* 2, 1865.

Progressive loco-motor Ataxy.—Dr. Böning, of Einbeck, reports two cases of idiopathic progressive loco-motor ataxy, in which very marked improvement was brought about, in one case by "expectant" treatment, in the other by the exhibition of nitrate of silver.

The case treated by the expectant method (purgatives being also employed) occurred in a woman æt. 60, who was unable to walk, or even to sit, unless she was held up on both sides. After treatment she could mount the staircase with the help of a walking-stick.

The other patient was a man of 49, whose illness dated from a cold caught five years previously. He could still walk, but his movements were very jerky, and he had extreme difficulty in keeping his balance. In six months he took ʒiss of silver, paused for three months, and then took it again for three months. His gait then presented nothing abnormal except to an experienced eye, and walking did not fatigue him. The improvement still remained seven months after treatment was discontinued.—*Deutsche Klin.* 1—5, n.s., 1865. *Schmidt's Jahrb.*, B. 126, p. 285, 1865.

The alleged connection between Aphasia and Right Hemiplegia, and Lesion of the External Left Frontal Convolution of the Brain.—Dr. Sanders has lately drawn attention to a case which seems at first sight to confirm M. Paul Broca's opinion that aphasia (inability to express thoughts by words) depends on lesion of the posterior third of the third, external or inferior left frontal convolution (étage surcilier ou frontal inférieur of Gratiolet). The case was that of a woman aged forty-three, who was admitted a patient of the Royal Infirmary on Nov. 16th, 1865, and died there on January 10th, 1866. Her principal symptom was defective speech and loss of the memory of words. She could speak a few words consecutively quite well, but, after commencing a sentence, she either stopped abruptly or concluded it in a manner altogether unintelligible. For instance, she gave her Christian name, but could not tell her surname. She called "druggist" "yollit," "retched" "rubbished," "Duncan and Flockhart," she made "Doctor and Forrit." Her hearers had to guess in great part what she said: she signified her assent when they guessed rightly, but shook her head when they interpreted her meaning wrongly. Her illness commenced with paralysis of the right hand and leg; when admitted, power of motion had been to a great extent regained. There was no distinct facial palsy; loss of power in the left leg, depending evidently on obstruction of the femoral artery, came on soon after her admission. This was followed by gangrene. The aphasia continued until the time of her death. She was unable to write her name; towards her end her intelligence became weakened. Examination twenty-seven and a half hours after death: "The skull-cap was natural; there was some serous subarachnoid effusion. The right hemisphere of the brain presented no lesion. On carefully examining the left hemisphere, the posterior part of the external or inferior left frontal convolution, where it forms the anterior margin of the fissure of Sylvius, together with a small portion of the adjoining orbital convolution, were observed to be collapsed, and depressed below the natural level. The flattened and depressed portions felt soft and fluctuating to the touch, like a bag containing fluid, presenting a marked contrast to the firmness of the neighbouring healthy parts of the brain. The gray matter of the diseased convolutions was smooth and intact on its external surface; but, on cutting into the softened part, the gray matter was found to be thinned off from within, and the white cerebral substance was completely softened and eroded, presenting an appearance like dirty cream. The softened part extended inwards for about three quarters of an inch, reaching to the immediate neighbourhood of the left corpus striatum, without, however, affecting it. Under the microscope the softened portions consisted entirely of granules and granular cells, the nervous structure being entirely destroyed. The other convolutions of the left anterior lobe were not affected, and the island of Reil, and the adjoining portions of the parietal and temporo-sphenoidal lobes, were healthy. Near the posterior extremity of the fissure of Sylvius, however, there existed a separate softened portion in the situation of the anterior and lower part of the lobule of the superior marginal convolution of Gratiolet. Like the former softening, this could be detected externally by the sinking of the convolution, and by the soft touch and feeling of fluctuation communicated to the finger. Here also the superficial gray matter was not destroyed externally, but internally it was softened, and the adjoining white nerve-substance was eroded and softened to the aspect of dirty cream over the extent of three quarters of an inch. The softening presented the same microscopical appearances as the portion already described. No other lesion of the encephalon was found; no embolism was detected in the arteries of the brain. The author does not lay undue weight on the remarkable coincidence between the post-mortem appearances in this case and M. Paul Broca's conclusions. He suggests that post-mortem examinations of the surface of the brain should for the future be made with greater accuracy, in reference to the system of convolutions as classified by Gratiolet. He remarks, that although there are authentic cases on record of loss of speech with left hemiplegia, yet in the immense majority of recorded cases the paralysis has been on the right side. —*Edinburgh Medical and Surgical Journal*, March, 1866.

Cerebral Ramollissement.—The following are some of the conclusions arrived at by MM. J. L. Prevost and J. Cotard, from a series of physiological experiments and pathological observations on softening of the brain (excluding cases of true inflammatory softening). By experiments on animals, by means of artificial emboli, they have produced *ramollissements* identical with those observed in man, and they have watched the progress of the disease in its different periods, from the commencement of hyperæmia to the necrobiotic degeneration which succeeds it, and onwards to the formation of connective-tissue, and the yellow *plaques* which appertain to the third period of the malady. They show that manifest congestion is produced at the points to which the obliterated artery is distributed, and that it is difficult in the existing state of science to account for the cause of this hyperæmia. Whatever may be its mechanical cause, they believe that the hyperæmia of red softening is of entirely a different character from that of inflammation. They have watched the process of degeneration from its commencement, and they find that as early as the third day there exist granular bodies and separate fatty granules in large numbers, chiefly around the capillaries, to which they form a kind of sheath. The walls of the capillaries themselves sometimes present a consecutive granulo-fatty degeneration, and in one case aneurismal dilatations were observed. Thermometric observation showed that the heat of the body was not elevated in softening of the brain, as in inflammation. They conclude, that if inflammation play any part in the disease it is certainly very secondary, and that the processes which essentially constitute it are of another nature.—*Gazette Médicale de Paris*, No. xxviii, July 14, 1866.

The Influence of the Nervous System in the Production and Prevention of Dropsies.—Dr. Laycock seeks to explain the localization of dropsical effusions by a reference to the limiting powers of the nervous system. He believes that there are distinct areas of vascular action and capillary distribution which have their corresponding nerve-centres (NEURO-VASCULAR AREAS), and which have limited inter-connection with each other. He refers to the fact noted by Andral, that the order of development in the anasarca of cardiac dropsy is not always the same; that although the legs usually swell first, and then the scrotum and penis, in some they become anasarcous at the same time; while in others the latter only become infiltrated when œdema is already manifested at many points. He also details numerous cases in which the localization of dropsical effusion could not be referred to the effect of gravitation, but rather in some of them clearly followed an opposite direction to that which would have been taken had effusion been entirely controlled by position. Now, these and similar anomalies may all, Dr. Laycock thinks, be explained by referring them to the influence of the nervous system on the tissues affected, nervous influence being shown by two distinct and apparently antagonistic results, namely, the prevention as well as the causation of watery effusion of serum. He therefore adopts an arrangement of the various kinds of dropsies founded on that of diseases of the nervous system. He speaks of centric and reflex dropsies, of general dropsies, and of paraplegic, hemiplegic, facial, and other local effusions, according as the disease extends over entire symmetrical regions, or is limited to a local system of nerves or nerve influence. The author suggests the probability of the existence of a trophical nervous system distinct from the sensory, volitional, and visceral, and having, like those systems, restricted centres and areas of action. He believes that the facts of the cases contained in his paper warrant the following conclusions:—1. That the nervous system as a whole, or else some special division of it, has a direct influence both on the production and prevention of anasarca. 2. That anasarca is produced when innervation is defective· 3. That anasarca is prevented being manifested locally, when the general causes are in operation, by more vigorous because more healthy innervation of the exempted tissues. 4. That centric disease or disorder may have the double effect of facilitating the effusion in one lateral portion of the body, and preventing it in the other lateral portion. 5. That production or prevention alike follow upon changes in innervation, which are induced in the same way and according to the same laws as other neuroses; and, 6. That it is not the sensory, motor, or vaso-motor systems which are specially involved.—*Edinburgh Medical Journal, March*, 1866. In a paper in the April number of the same journal Dr. Laycock discusses the "Conjoined Influence of the Nervous System and of Constitutional Tissue-changes in the Production of Dropsies, and the Subject of Treatment."

Tuberculosis of the Pharynx. By Dr. B. Wagner.—Besides tuberculosis of the lungs, liver, spleen, kidneys, and so forth, there were at the root of the tongue, and on the anterior of the surface of the soft palate, numerous ulcers, as large as lentils; the surrounding tissues were whitish, and thoroughly infiltrated. The rest of the mucous membrane of the base of the tongue, as well as that of the upper half of the fauces and its sides, showed prominent reddish-gray granulations, of the size of millet and hemp seeds, and separate, flat ulcers, some circular, some irregular-shaped through confluence, covered with cheesy pus. The glosso-epiglottidean and the ary-epiglottidean ligaments were partially destroyed by ulceration. The mucous membrane of the larynx and the trachea contained numerous miliary tubercles and ulcers. Microscopical examination showed that the granulations consisted of the elements of tubercle, small cells and nuclei, for the most part shrunken and in a state of degeneration, and chiefly fine, fatty, detritus. The mucous membrane of the pharynx showed a prolific production of nuclei, accumulated round the greatly enlarged mucous glands. The follicles were little or not at all swollen. The development of the tubercle seemed here also to proceed from the connective-tissue; the corpuscles of that tissue were numerous, and for the most in the act of dividing.—*Arch. de Heilk.* vi., 5, p. 470, 1865). *Schmidt's Jahrb.* Bnd. 129, p. 300, 1866.

Pulsation of the Vena Cava inferior from Incompetency of the Tricuspid Valve.—The following cases noted by Von Seidel show that the venous pulse of tricuspid regurgitation may be perceived even in the vena cava inferior.

1. A woman, twenty-two years of age, who had suffered from heart disease for some years, was admitted with great œdema of the feet, ascites, extended precordial dulness, small pulse, a double murmur at the apex of the heart and over the right ventricle; stenosis and incompetency of the mitral; cyanosis of the neck existed without venous pulsation. 7,600 cubic centimètres of fluid were drawn off from the abdomen by tapping; during the operation the liver was felt, on lateral compression, pulsating so forcibly as to give rise to a suspicion of aneurism; the pulsation remained till the effusion collected anew, and it returned again after a second tapping.

Post-mortem examination confirmed the diagnosis. The inferior vena cava and the veins of the liver were greatly dilated. The abdominal aorta scarcely admitted the forefinger.

2. A woman, forty-eight years of age, exhibited long-standing heart disease and great dropsical effusion, ascites especially of extreme extent. Examination showed disease of the mitral valve, and great stenosis and incompetency of the tricuspid valve, with venous pulse in both internal jugulars. By paracentesis abdominis 9,300 cubic centimètres of water were drawn off, and as it flowed the same symptom was observed as in the former case. The pulsation became fainter again as water collected anew between the liver and the abdominal wall.

3. A woman, aged forty-eight, who had for years suffered from stenosis and incompetency of the mitral valve, and stenosis of the aorta, was admitted generally dropsical and nearly moribund; a short time previously symptoms of incompetency of the tricuspid valve had appeared, among others pulsation of the right internal jugular vein and loud murmurs in both internal jugulars. To the right of the navel, under the greatly enlarged liver, there could be seen and felt a deep-seated pulsation, which appeared to proceed from a soft body. Post-mortem examination corroborated the diagnosis; the right internal jugular was dilated; the inferior vena cava

greatly dilated.—*Deutsche Klin.* 9, 1865. *Schmidt's Jahrb.* B. 126, p. 170, 1865.

On Peritoneal Friction-sounds. By Dr. SEIDEL.—Gerhardt, Friedreich, and Mosler, lately brought forward some communications on the existence of a peritoneal friction sound, and Seidel now records that he has observed it in fifteen cases.

An emaciated man, aged fifty-six, had enlargement with palpable protuberance of the liver. A to-and-fro rubbing sound, coincident with the respiratory movements, could be heard when the ear was lightly applied over the left lobe of the liver, as well as plainly felt by the hand. Effusion into the abdominal cavity, collapse, and death quickly followed. Examination of the body showed, with other morbid conditions, numerous medullary tumours in the greatly enlarged liver. In the left lobe some small tumours had pierced the peritoneum, and the peritoneal coat in the neighbourhood was roughened by delicate pseudo-membranous exudation.

An exactly similar sound could be heard and felt over the liver, after two abdominal tappings, in a woman, who also suffered from eccentric hypertrophy of the right breast. After a third tapping, after which effusion returned more slowly, and the murmur appeared later, a similar rubbing sound, coincident with the respiratory movements, could be very distinctly heard and less distinctly felt over the stomach. If the breath was held for a while the rhythm of the murmur became synchronous with the movements of the heart. On post-mortem examination several pounds of brownish serum were found in the abdominal cavity. The parietal peritoneum was thickened and beset with small extravasations; upon it, and between the viscera, were flocculent membranous exudations.

Shortly after the performance of the Cæsarean section a delicate rustling sound, like the vesicular murmur, could be heard over the uterus; it followed the respiratory movements. The necropsy showed a very delicate fibrinous exudation over the uterus.

The other cases report similar murmurs over the spleen in intermittent fever and in tuberculosis, over the intestines in acute and chronic peritonitis, over the hypogastric region in carcinoma of the bladder, &c.

Dr. Seidel remarks that the murmur generally indicates a chronic, more rarely an acute, process; that it is not confined to the neighbourhood of the fixed viscera; that it is generally rhythmical, the influence of respiration being evident even in the hypogastric region; that a "perihepatic" murmur, especially when superficial, may be easily confounded with a pleuritic rubbing sound; that the murmur varies in tone from a gentle rustling to the hardest creaking; that no similar sound is produced by the peristaltic movements.—*Deutsche Klin.* 49 u. 51, 1865; *Schmidt's Jahrb.* B. cxxx, pp. 33-4, 1866.

Atrophy of the Spleen.—Dr. C. Kuttner relates the case of a man, aged sixty, who was admitted into hospital July 8th, 1863, on account of cancer of the lip. He was emaciated, and there was a considerable deposition of pigment in the skin. The veins of the abdominal walls on the left side were considerably developed. He died on August 8th. The post-mortem revealed hypertrophy of the liver and congestion of the kidneys. The spleen was so reduced in size that it was found with difficulty; it was not larger than a walnut, and was ossified. The author attributes this atrophy to marasmus, and he refers the pigmentation of the lungs, the bronchial glands, the muscles, and the skin, to the cessation of the functions of the spleen. The works of Virchow lead to the opinion that pigment proceeds from a modification of the colouring of the blood, and that the red corpuscles are directly transformed into pigment-granules. The spleen is the organ where this modification takes place physiologically, but pathologically it takes place in the parenchyma of other organs; but whereas pigment produced in the spleen is carried to the liver, and is eliminated with the bile, pigment formed in the other organs remains in them and colours them. We can thus conceive that extirpation of the spleen in animals is not necessarily fatal.—*Petersburger Medicinish Zeitschrift and Gazette Médicale de Paris*, August 19, 1865.

Rupture of the Aorta.—Since the publication of M. Broca's Memoir in the 'Bulletins de la Société Anatomique,' 1859, several instances of this rare accident have been recorded, which have been collected and analysed by Dr. Chauvel. He has arrived at the following results. *Sex.* Of 18 cases collected by Broca, 9 were male and 9 female; of 12 collected by Chauvel 10 were male, 2 female. The lesion is therefore most frequent in men. *Age.* In 15 of Broca's cases, where the age was noted, 2 (females) were 29; 6 between 30 and 40; 4 between 40 and 60, and 3 upwards of 60 years old. Out of 14 cases, in which the exact age was not noted, there were 2 aged men, and 1 young woman. The others are designated as "man of mature age," "man in the vigour of age. M. Broca, therefore, concludes that the lesion is most common between the ages of 30 and 60. Of 12 cases collected by Chauvel 2 were under 30 years, 4 between 40 and 60, and 6 above 60. Chauvel, therefore, finds that rupture of the aorta is more common in old age than in mid life. *Seat.* In 28 cases (Broca) the seat of the rupture was in 19 in the pericardiac portion; in 7 in the thoracic aorta outside the pericardium; in 2 in the abdominal aorta. In 12 cases (Chauvel) the seat of rupture was in 10 within the pericardium; in 1 in the thoracic aorta outside the pericardium; in 1 in the abdominal aorta. It is therefore evident that the most common seat of the lesion is in the pericardiac portion of the vessel. *Direction.* Sometimes oblique, sometimes longitudinal, but most frequently the rent has been found transverse (5 times in 8). *Extent.* Very variable, sometimes scarcely visible, sometimes involving nearly the whole circumference of the artery. In one case there was tearing of the two internal tunics at two distinct points. In another a single rupture of the internal and middle coats, corresponding to a double rupture of the cellular coat. The three arterial tunics are rarely torn at the same level and to the same extent. *Condition of the arterial walls.* Broca, following Morgagni, believes that rupture cannot take place unless favoured by an alteration in the arterial coats. Of 12 cases collected by Chauvel, in 3 the artery was said to be healthy; in 1 the rupture was the result of external injury; the two others were men of 42 and 55 years, the one paraplegic, the other emphysematous, who died suddenly in bed without effort and without any external violence. In both the rupture was considerable; in 1 it occupied a third of the circumference of the vessel; in the other it had an extent of 8 centimètres. In all the other cases alteration of the arterial walls was noted, but in varying degree. Atheromatous degeneration was met 6 times in 9, accompanied almost always by a dilatation of the artery. All these cases were over 60 years of age. Of the three other cases, in one a cancerous tumour was the cause of rupture, in another softening of the middle tunic, which had become gray, soft, friable, and pulpy; in another there was almost complete obliteration of the vessel preceded by a considerable dilatation. *Direct causes.* Of 17 cases collected by Broca, in 2 (females) lively emotion appeared to be the cause of the accident; in 1 a large meal; in 7, efforts or paroxysms of anger; and in 7 external violence. In the twelve observations collected by Chauvel 2 cases were due to external violence, 1 to a paroxysm of anger, and 2 to effort. In the 7 other cases no manifest cause was recorded, but in 4 the circulation was disordered by pulmonary or cardiac affection. In 16 of M. Broca's cases death was immediate, in the other 9 it took place in from a quarter of an hour to 14 days after the accident. In Chauvel's 12 cases, in 8 death was immediate, in the other 4 death occurred in from 16 hours to 15 days after the rupture. In all the latter, and in four of the former, the rent in the internal and middle tunics took place at a point higher than that in the cellular coat. In 8 cases, and especially in 1 observed and recorded by the author, intense pain at the lower part of the sternal region signalised the lesion. "On Ruptures of the Aorta by Dr. Chauvel."—*Gazette Medicale de Paris*, July 8 and 15.

On Typhoid Pneumonia associated with Muffled Tympanitic Resonance.—Dr. Hayden has called attention to the phenomenon of tympanitic resonance on percussion occurring over solidified lung in the course of typhoid pneumonia. He records two cases in which this phenomenon was observed by himself; one of these (No. I) ended in recovery, the other (No. II) in death. In the former of these cases the upper lobe of the left lung only was hepatized: the tympanitic resonance occurred on the fourteenth day of the illness; it was limited to the left subclavicular region. In the latter the whole right lung was hepatized, the peculiar percussion note was developed over the space from the nipple to the clavicle. It was observed five days before death. It is thus described:—"From nipple to clavicle the percussion note is of a mixed and very singular character; it is that of dulness qualified by a metallic resonance, and communicates the sensation of a solid but resonant body under percussion. Over the region which presents this remarkable modification of dulness respiration is bronchial, accompanied by crepitus on full inspiration." Subsequently, in the progress of the case, it was observed that the right side of the chest was more prominent in front than the left; it was also observed that there was less respiratory movement on that side. After death the chest was examined under water. On the cavity of the pleura being laid open no air escaped, but on puncturing the hepatized lung bubbles of air freely escaped. The lung was adherent to the chest walls and to the diaphragm. On percussing the superior and middle lobes of the hepatized lung, the tissue yielded a somewhat muffled metallic ring, a shade less clear than that yielded by the corresponding part of the patient's chest during the last three days of his life; the percussion note elicited from the anterior edge of the lung placed upon the hand was similar, *i. e.*, muffled metallic. Percussion of the inferior lobe, at all points of its surface, yielded a perfectly dull sound. The author gives, for the sake of comparison, another fatal case of pneumonia (No. III), in which the upper lobe of the right lung only was hepatized, the lower being unimpaired; no tympanitic percussion sound was elicited in this case. The following conclusions are deduced. "1st. The phenomenon is not due to transmitted resonance from a healthy through a solidified portion of lung substance. *a.* Because in Case II, in which it was best pronounced, the *entire* lung was solid. *b.* Because in Case III it did not exist, although the inferior and posterior portion of the lung was physically healthy. 2nd. It was not the result of gastric resonance transmitted through a solid lung. *a.* Because it existed in Case I, in which only the superior portion of the left lung was hepatized, the inferior lobe being in a healthy condition. *b.* Because in Case II it existed only in the superior portion of the right lung, notwithstanding that the entire organ was solid; and it likewise existed in the *isolated* lung, as proved by *post-mortem* test. 3rd. It was not due to pneumothorax. *a.* Because in Case II it existed up to death, and after death the lung was found universally adherent to the chest, and no air existed in the pleura. 4th. The phenomenon was intrinsic in the lung, and had its seat in that portion of the organ in which it was manifested. *a.* Because percussion of the lung removed from the body afforded proof of its existence in the isolated organ, and even in a thin layer of it resting on a solid body. *b.* Because percussion showed dulness in that portion of the lung, after removal from the body, over which dulness existed during the patient's illness. 5th. The resonance of the solidified lung was associated with the presence of air in its tissue. *a.* Because air freely escaped from an opening made with the finger, under water, in that portion of the lung which yielded tympanitic resonance. 6th. Simple pneumothorax, whether pneumonic or pleuritic, is characterised by absence of respiratory sound, coextensive with tympanitic resonance, and by displacement of the heart if the aëriform effusion be abundant. 7th. The resonant or tympanitic dulness of pneumonia, due to an implication in the tissue of the lung, is distinguished from pneumothorax by the qualified or muffled character of the resonance, and by the presence of bronchial respiration and of crepitus."—Dr. T. Hayden. *Dublin Quarterly Journal*, Feb. 1866. No. LXXXI.

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

Adversaria Medico-Philologica.

BY W. A. GREENHILL, M.D. OXON.

Part V.

(*Continued from* vol. xxxvii, p. 279.)

[Those persons who may consult this collection of words are requested to bear in mind that it does not pretend to completeness in any way. It is very far from being a complete collection of Greek medical terms, for all botanical and chemical words have been designedly omitted; neither is the treatment of each word to be considered complete, for, no doubt, various meanings are passed over, and probably better passages might frequently be quoted. It is simply a contribution (and that a very imperfect one) to Greek medical technology.]

ἄσφυκτος, a person whose pulse is so weak, that it cannot be felt in any part of his body,[1] though, if it had ceased entirely, he would no longer be alive.[2]

ἀσφυξία does not signify *asphyxia*, in the modern sense of the term, but simply, as the word imports, *pulselessness*. It is used by Aretæus,[3] and other authors, and is explained by Galen to apply only to those cases in which no pulsation is felt in any part of the body; though he adds, that, strictly speaking, this could not occur to any person during life.[4] He distinguishes *ἀσφυξία* from *σφυγμὸς ἐκλείπων*,[5] or *failing pulse*, as being a more complete pulselessness.

ἀσώδης, *subject to nausea* (*ἄση*), applied to a patient or a disposition (*διάθεσις*),[6] or *accompanied by nausea*, applied to *ὀδύνη*, *πυρετός*, *ἔμετος*, *ἀγρυπνία*, and other similar words.[7]

ἄτακτος, *irregular*, or *disorderly* (in Latin, *inordinatus*), when applied to the pulse, is opposed to *τεταγμένος*, *regular*, or *orderly*, and distinguished from *ἀνώμαλος*, *irregular*, or *uneven* (in Latin, *inaequalis*), inasmuch as a pulse that is *ἄτακτος* is always *ἀνώμαλος*, but a pulse that is *ἀνώμαλος* is not necessarily *ἄτακτος* also. For instance, when any number of strong pulsations are regularly succeeded by a feeble one, or when the pulse is regularly intermittent, its equality (*ἰσότης*) is lost, and it becomes *ἀνώμαλος*; but, inasmuch

[1] Galen, 'De Differ. Puls.,' iv, 8, tom. viii, p. 720, l. 13.

[2] Id., *ibid.*, p. 725, l. antepen.

[3] 'De Caus. Morb. Acut.,' ii, 11, p. 63, l. 13, ed. Kühn.

[4] 'De Præsag. ex Puls.,' i, 3, tom. ix, p. 227, l. 14; p. 228, l. 4.

[5] 'De Caus. Puls.,' ii, 3, tom. ix, p. 66, l. 13.

[6] Galen, 'De Compos. Medic. sec. Loc.,' viii, 1, tom. xiii, p. 122, l. 5.

[7] See Foës, 'Œcon. Hippocr.,' under *ἄσαι*.

as a certain order (τάξις) is preserved, it is still considered τεταγμένος. When, however, the pulse has no order in its irregularity, it is then called ἄτακτος, *disorderly*.[1]

ἄτακτος πύρετος is a fever that is irregular in its course, and follows no certain type.[2]

ἀτάκτως, the adverb, is also applied to fevers,[3] sometimes opposed to εὐτάκτως,[4] sometimes to τεταγμένως,[5] and in Hippocrates joined with πεπλανημένως.[6]

ἀταξία, the substantive, is applied in the same sense to the pulse.[7]

ἄτλας, the name applied by Julius Pollux[8] to the *last* of the cervical vertebræ, ὡς ἀχθοφορῶν, as bearing the burden of the head and neck. Possibly, he is the only ancient writer who uses the word with reference to the vertebræ. In modern works it is applied to the *first* cervical vertebra, which is called by Julius Pollux[9] ἐπιστροφεύς, and by Galen[10] and others simply ὁ πρῶτος σπόνδυλος.

ἀτονία, a word as old as Hippocrates, who uses it to signify *relaxation*, or *want of tone* in the system generally, as if nearly synonymous with μαλακίη.[11] Demetrius of Apamea, in the third century B.C., applied it, perhaps, especially to the coats of the veins, and reckoned ἀτονία (rendered by Cælius Aurelianus[12] *defectio vel debilitas corporum*), among the four causes of hæmorrhage without a wound. It was one of the words used improperly (according to Galen) by the Methodici, who apparently spoke of it as if it were an actual disease, instead of merely an inability in any part fitly to perform its proper functions (κατὰ τὴν ἐνέργειαν ἀρρωστία).[13]

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[1] Galen, 'De Puls. ad Tir.,' c. 6, tom. viii, p. 458. See also Index to Kühn's Galen, under 'Pulsus;' and Adams's 'Commentary on Paulus Ægineta,' ii, 12, vol. i, p. 215.

[2] Galen, 'De Typis,' cc. 2, 4, tom. vii, p. 464, l. 4; 471, 8, 16.

[3] Id., *ibid.*, p. 470, l. 13; p. 471, l. 10.

[4] Hippocrates, 'Epid.,' i, tome ii, p. 620, ll. 4, 7, ed. Littré.

[5] Galen, *ibid.*, p. 471, l. 11.

[6] *Loco cit.*, p. 624, l. 3; p. 626, l. 5.

[7] Galen, 'Defin. Med.,' cap. 217, tom. xix, p. 407; Aretæus, 'De Caus. Morb. Acut.,' ii, 10, p. 59, l. 13, ad Kühn. In this latter passage Wigan reads ἀτάξιοι, as if from ἀτάξιος, and this word appears in Maittaire's 'Index Graecus,' but with an asterisk to signify that it is not elsewhere to be met with. The correct reading is, however, ἀταξίῃ, which has been adopted by Ermerins and Adams.

[8] 'Onom.,' ii, 4, §§ 132, 178.

[9] *Ibid.*, § 131. There is a note on this word in Kühn's "Cens. Medic. Lexic. Recent.,' in his 'Opusc. Acad. Med. et Philol.,' vol. ii, p. 328.

[10] 'De Oss.,' cap. 8, tom. ii, p. 756, l. 14, quoted by Oribasius, 'Coll. Med.,' xxv, 9, § 7, tome iii, p. 405, l. 12. Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 67, l. 15, and others.

[11] 'De Aëre, Aquis et Locis,' c. 20, tome ii, p. 74, ll. 4, 6.

[12] 'Morb. Chron.,' ii, 10, § 123, p. 391, ed. Amman.

[13] 'De Morb. Differ.,' c. 5, tom. vi, pp. 853, 854; 'De Meth. Med.,' ii, 4; iv, 4, tom. x, pp. 102, 103, 270.

ἄτονος, *wanting in tone, relaxed*,[1], the opposite to which is ἔντονος,[2] or εὔτονος.[3]

ἀτράχηλος, *without a neck*, as applied to a fish; *with a short neck*, as applied to a man;[4] or synonymous with κακοτράχηλος.[5]

ἄτρητος, *imperforate*, applied to a part of the body, as the *anus*,[6] the *meatus auditorius*,[7] the *uterus*;[8] sometimes applied to the person affected.[9] It is also found joined to πάθος,[10] and signifies the affection itself, *imperforation*, or *atresia*, which latter word is not found in the Greek writers.

ἀτροφέω, to be subject to ἀτροφία.[11]

ἀτροφία, *want of nutrition*; *atrophy*, is reckoned by Celsus,[12] as one of the species of *tabes*, the other two being καχεξία and φθίσις. He describes it as a condition "in which the body is not nourished, and in which (as some particles are constantly being lost, while no others supply their place,) extreme emaciation comes on, and eventually proves mortal." Cælius Aurelianus[13] also has a chapter on the subject of ἀτροφία, or "nutrimenti cessatio;" but the word is not always used in any such definite sense, and ἀτροφία was sometimes considered as a symptom only, not as a disease.[14] It was applied either to the whole of the body, or to any particular part,[15] as the hair,[16] or eye,[17] or leg.[18]

ἄτροφος, *affected with* ἀτροφία;[19] also, *containing little nourishment*.[20]

ἄτυπος, applied to a fever *that has no regular form or type*.[21] In Aulus Gellius (if the reading is correct) it is joined with *balbus*, and appears to mean a person *who stammers, or speaks inarticulately.*[1]

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[1] Hippocrates, 'De Aëre, Aquis et Locis,' cc. 3, 19, tome ii, p. 16, l. penult.; p. 72, l. 14, ed. Littré; Galen, 'De Meth. Med.,' ii, 4, tom. x, p. 102, l. 16; 'De Comp. Medic. sec. Loc.,' viii, 3, tom. xiii, p. 147, l. 1.

[2] Hippocrates, *ibid.*, cc. 4, 20, p. 18, l. ult.; p. 74, l. 7.

[3] Id., 'Aph.,' iii, 17, tome iv, p. 494, l. 1; Galen, 'De Comp. Medic. sec. Loc.,' viii, 3, tom. xiii, p. 146, l. ult.

[4] Galen, 'De Hipp. et Plat. Decr.,' iv, 4, tom. v, p. 384, ll. 3, 4, &c.

[5] Paulus Ægineta, ii, 11, p. 16, l. 37, ed. Ald.

[6] Id., vi, 81, p. 330, ed. Briau.

[7] Id., vi, 23, p. 140.

[8] Soranus, 'De Morb. Mul.,' p. 214, l. 9, ed. Dietz.

[9] Pseudo-Galen, 'Introd.,' cap. 19, tom. xiv, p. 787, l. 9, where it refers to the *glans penis*; Soranus, p. 14, l. 4; p. 216, l. 25, and Paulus Ægineta, vi, 72, p. 294, where it refers to the female organs of generation.

[10] Soranus, cap. 5, p. 13, l. 18; p. 14, l. 1, quoted by Oribasius, 'Coll. Med.,' xxiv, 32, §§ 5, 7, tome iii, p. 380, ll. 4, 12.

[11] Galen, 'De Sympt. Differ.,' cap. 4, tom. vii, p. 71, ll. 6, 9, 12; p. 73, l. 8.

[12] 'De Med.,' iii, 22, p. 154, ed Targa.

[13] 'Morb. Chron.,' iii, 7, p. 465, ed. Amman.

[14] Galen, 'De Meth. Med.,' i, 9, tom. x, p. 68, l. 12.

[15] Id., 'De Sympt. Differ.,' cap. 4, tom. vii, p. 70, l. ult.

[16] Id., 'Defin. Med.,' cap. 310, tom. xix, p. 430.

[17] Id., *ibid.*, cap. 342, p. 435; Pseudo-Galen, 'Introd.,' cap. 16, tom. xiv, p. 769, l. 17.

[18] Cælius Aurelianus, 'Morb. Chron.,' v, 1, p. 548.

[19] Galen, 'De Sympt. Differ.,' cap. 4, tom. vii, p. 71, l. 14; p. 72, l. 4.

[20] Id., 'De Alim. Facult.,' i, 2, tom. vi, p. 484, l. 4.

[21] Id., 'De Typis,' c. 4, tom. vii, p. 471, ll. 5, 14.

αὔξησις, *increase*, when applied to a disease, means the second period (sometimes called *ἀνάβασις*, or *ἐπίδοσις*), in which it is increasing in severity; the other stages being the *ἀρχή*, *ἀκμή*, and *παρακμή*.[2] When applied to an animal or a plant, it signifies *growth*, which is defined to be its increase in length, breadth, and depth:[3] this was considered to be the second of the three principal operations of nature, the other two being *γένεσις*, *generation*, and *θρέψις*, *nutrition*.[4]

αὐξητικὴ δύναμις, the power or faculty by which nature brings about *αὔξησις*, *growth*.[5]

αὐστηρός, *of a harsh, rough taste*.[6]

αὐστηρίζω, *to be of a harsh, rough taste;* omitted in Liddell and Scott's Lexicon.[7]

αὐστηρότης, *harshness, roughness of taste*.[8]

αὐτόπυρος, a word used by Celsus,[9] reckoned as comparatively modern in Galen's time, and equivalent to the more ancient term, *συγκομιστός*.[10] It means *wheaten flour with the bran in it*, and also, *the bread made from it*, which last is also called *αὐτοπυρίτης*.[11]

αὐχήν, *the neck*,[12] synonymous with *τράχηλος*;[13] sometimes signifying especially the back part only.[14] Probably, in medical writings, it most frequently signifies *a neck* in a secondary or metaphorical sense, and is joined to some other word: as *ὁ τῆς κύστεως αὐχήν*, *the neck of the bladder*,[15] which is sometimes called *τράχηλος*,[16] *ὁ τῆς ὑστέρας αὐχήν*, *the neck of the uterus*, generally synonymous with τράχηλος,[1] sometimes distinguished from it.[2] When ἀυχήν relates to the bones, it is explained by Galen to mean an apophysis terminating in a larger and spherical extremity called the *head*, κεφαλή, and is distinguished from an apophysis which terminates in a point and which is called κορώνη.[3] ὁ κατὰ τὸν ἀστράγαλον αὐχήν, means probably what is still called the *neck of the astragalus*.[4] ὁ αὐχήν τῆς κάτω γέννος,[5] means the *neck of the lower jaw-bone* supporting the condyle. ὁ αὐχὴν τοῦ μηροῦ, *the neck of the femur*.[6] ὁ αὐχὴν τῆς ὠμοπλάτης, or τῶν ὠμοπλάτων, *the neck of the scapula*.[7] In a passage of Nicander αὐχήν signifies the *œsophagus*.[8]

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[1] 'Noct. Att.,' iv, 2, § 5.

[2] Galen, 'De Cris.,' i, 2, 3, 8, tom. ix, p. 551, l. 2; p. 556, l. 14; p. 581, l. 2; 'De Morbor. Tempor.,' c. 2; 'De totius Morbi Tempor.,' cc. 1, 3, tom. vii, p. 411, l. 11.

[3] Id., 'Defin. Med.,' c. 202, tom. xix, p. 378, l. 14; 'De Natur. Facult.,' i, 5, tom. ii, p. 11, l. 4; 'Comment. in Hippocr. "De Alim.,"' ii, 1, tom. xv, p. 230, l. 4.

[4] Id., *ibid.*, tom. ii, p. 10, l. 12; p. 20, l. 1; tom. xv, p. 229, l. ult.

[5] Id., *ibid.*, tom. ii, p. 16, l. 1; tom. xv, p. 226, l. 13; p. 230, l. 18.

[6] Oribasius, 'Coll. Med.,' i, 49, § 6, tome i, p. 63, l. 3, where there is a good note by Dr. Daremberg (p. 579).

[7] Anon., 'De Cibis,' c. 4, in Ermerins, 'Anecd. Med. Gr.,' p. 235, l. 16.

[8] Galen, 'Defin. Med.,' c. 462, tom. xix, p. 458, l. 2.

[9] 'De Med.,' ii, 18, p. 91, ed. Targa.

[10] 'De Alim. Facult., i, 2, tom. vi, p. 483, l. 1; 'Comment. in Hippocr. "De Acut. Morb. Victu,"' ii, 34, tom. xv, p. 577, l. 3.

[11] Id., tom. vi, p. 484, l. 3.

[12] Aristotle, 'Hist. Anim.,' i, 10, § 1, and elsewhere.

[13] Galen, 'De Oss.,' cap. 7, tom. ii, p. 756, l. 2; Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 28, ll. 14, 15, ed. Clinch.

[14] Id., *ibid.*, p. 50, l. 5.

[15] Id., 'De Usu Part.,' v, 8, tom. iii, p. 375, l. 10, quoted by Oribasius, 'Coll. Med.,' xxiv, 28, § 1, tome iii, p. 364, l. 7; Galen, *ibid.*, xv, 3, tom. iv, p. 222, l. 14; p. 228, l. 1, quoted by Theophilus, 'De Corp. Hum. Fabr.,' v, 24, §§ 2, 3, p. 220, ll. 3, 8, ed. Oxon.

[16] Id., *ibid.*, xiv, 9, tom. iv, p. 182, l. penult., quoted by Theophilus, *ibid.*, v, 36, § 4, p. 260, l. 3.

αὐχμηρὸς σφυγμός, *a dry pulse*, is opposed to ὑγρὸς σφυγμός, *a moist pulse*. Perhaps the terms are only to be found in one passage,[9] where the text is corrupt, and where neither word is defined in terms quite intelligible to a modern reader:—a *hard pulse* probably gives some idea of what was intended.

ἄφθα, a word sometimes found in the singular form,[10] but much more frequently in the plural (ἄφθαι). It was generally applied to ulcers of the mouth, in the modern sense of the word *aphthae*, or *thrush*, but also sometimes to similar ulcers in other parts of the body, as may be gathered from the use of the kindred words ἀφθάω and ἀφθώδης. It was very long before the word was Latinized, as not only Celsus, in the first century, speaks of "ulcera oris quae ἄφθας Graeci nominant,"[11] but even as late as the fourth or fifth century Marcellus Empiricus writes in the same way.[12] The word is of frequent occurrence.[13]

ἀφθάω, *to be affected with* ἄφθαι, a word probably always used with reference to the *part affected*, not to *the patient*.[14] It relates to various parts of the body, as the *trachea*,[15] the *testicles*,[16] and the

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[1] Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 40, l. penult.

[2] Soranus, 'De Morb. Mul.,' cap. 4, p. 8, ll. 16, 17, quoted by Oribasius, 'Coll. Med.,' xxiv, 31, § 8, tome iii, p. 372, ll. 8, 9. See the Notes to the Oxford edition of Theophilus on p. 208, l. 5, and p. 209, l. 9.

[3] Galen, 'De Oss.,' procem., tom. ii, p. 736, l. 8, quoted by Oribasius, 'Coll. Med.,' xxv, 2, § 3, tome iii, p. 393, ll. 1, 4.

[4] Galen, 'De Anat. Adm.,' ii, 10, tom. ii, p. 333, l. 18.

[5] Theophilus, 'De Corp. Hum. Fabr.,' iv, 29, §§ 9, 11, p. 180, l. 11, p. 181, l. 2. See Galen, 'De Oss.,' procem., tom. ii, p. 733, l. 15.

[6] Galen, 'De Anat. Adm.,' ii, 5, 6, tom. ii, p. 303, l. 12; p. 313, l. 8; id., 'De Usu Part.,' iii, 9, tom. iii, p. 211, l. 11; Theophilus, *ibid.*, i, 23, § 6, p. 58, l. 3. See also Hippocrates, 'De Artic.' § 55, tom. iv, p. 240, l. 7, ed. Littré.

[7] Id., 'De Usu Part.,' xiii, 9, tom. iv, p. 120, l. 4; p. 122, l. 11, quoted by Theophilus, *ibid.*, v, 8, § 1, p. 197, l. 4; 9, § 2, p. 198, l. 12.

[8] 'Ther.,' v, 350, quoted by Liddell and Scott.

[9] Galen, 'Defin. Med.,' c. 211, tom. xix, p. 405, l. 12.

[10] Hippocrates, 'De Morb. Mul.,' i, § 40, tome viii, p. 96, l. 10, ed. Littré; Galen, 'Defin. Med.,' c. 381, tom. xix, p. 441, l. 8; Leo, 'Consp. Med.,' iv, 4, in Ermerins, 'Anecd. Med. Gr.,' p. 153.

[11] 'De Med.,' ii, 1, p. 43; vi, 10, p. 349, ed. Targa.

[12] 'De Medicam.,' c. 11, p. 292 A, ed. H. Steph.

[13] See Adams's Commentary on Paulus Ægineta, i, 10, vol. i, p. 15.

[14] In Hippocrates, 'De Morb. Mul.,' i, 34, Littré makes the *patient* the nominative to the verb (tome viii, p. 82, l. 9), but Ermerins has a different reading (vol. ii, p. 566, l. 3).

[15] Hippocrates, 'De Morb.,' ii, 50, tome vii, p. 76, l. 8, ed. Littré.

[16] Galen, 'De Comp. Medic. sec. Loc.,' ix, 8, tom. xiii, p. 817, l. 14; which

female organs of generation;[1] probably also to the *mouth*, though I have not found such a passage.

ἀφθώδης, a word found in the 'Coacae Praenotiones,' and therefore older than Hippocrates; probably never applied to a person, but to a part of the body, or to a disease, &c. Sometimes it means *like aphthae*, as ἀφθῶδες ἕλκος;[2] sometimes *complicated with aphthae*, as ἀφθῶδες ῥεῦμα;[3] sometimes *affected with aphthae*, as ἀφθῶδες στόμα.[4]

ἀφωνέω, *to be ἄφωνος*, whether *quite speechless*, or only *unintelligible*.[5]

ἀφωνία, *loss of voice*, meant sometimes nothing more than an excessive amount of hoarseness (βράγχοι), due to the same causes,[6] and synonymous with φωνῆς ἀποκοπή, or ἀποκεκομμένη φωνή.[7] Sometimes it was used to signify a total loss of voice depending on injury to the recurrent nerves,[8] or other causes, when it was synonymous with φωνῆς ἀπώλεια.[9] Sometimes in the Hippocratic Collection it is used, though only indicating a symptom, to imply the disease on which this symptom depends, *e. g.*, apoplexy, &c., as is explained by Galen in several passages.[10] There is a chapter in Leo, περὶ ἀφωνίας, in which it is treated only as the result of disease or of surgical injury.[11]

ἄφωνος, *without voice*, applied sometimes to a fish, or plant, or a person suffering from absolute loss of voice (στέρησις φωνῆς), sometimes in a less extensive meaning to a person *with some affection of the voice* (κάκωσις φωνῆς).[12] In this latter sense it is synonymous with ἀποκεκομμένος τὴν φωνήν.[13] In two passages Hippocrates uses the two words ἄναυδος and ἄφωνος in conjunction,[14] where ἄφωνος is considered by Galen[15] to be a stronger word than ἄναυδος, the latter signifying *unable to articulate*, the former *unable to utter a sound*. This explanation has been adopted by Foës[16] and Littré, and sanc-

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passage is found also in Oribasius, 'Ad. Eunap.,' iv, 108, p. 668, ed. H. Steph., and in 'Paulus Ægineta,' iii, 54, p. 51, l. 3, ed. Ald.

[1] Hippocrates, 'De Nat. Mul.,' §§ 60, 86, 100, tome vii, pp. 398, 408, 416.

[2] Galen, 'De Simpl. Med. Temp. ac Fac.,' vii, 10, § 65, tom. xii, p. 54, l. penult.

[3] 'Coac. Praenot.,' c. 31, §§ 518, 528, tome v, p. 704, ed. Littré, where it relates to the female genital organs.

[4] Hippocrates, 'Epid.,' iii, 3, §§ 3, 7, tome iii, p. 70, l. 6, p. 84, l. 2; 'Coac. Praenot.,' c. 31, §§ 504, 533, tome v, pp. 700, 706. In the last two passages στόμα probably means the *os uteri*. See Daremberg's Note 182, 'Œuvres d'Hippocr.,' p. 290, 2nd ed.

[5] Id., 'Epid.,' i, case 13, tome ii, p. 714, l. 1, ed. Littré.

[6] Galen, 'De Comp. Med. sec Loc.,' vii, 1, tom. xiii, p. 4, l. 16.

[7] Id., *ibid.*, p. 47, l. ult.; p. 48, l. 10; p. 35, l. 6.

[8] Id., 'De Anat. Admin.,' viii, 4, tom. ii, p. 675, ll. 2, 9.

[9] Id., 'De Locis Affect.,' i, 6, tom. viii, p. 53, l. 5.

[10] See Foësii 'Œcon. Hippocr.'

[11] 'Consp. Med.,' iv, 15, in Ermerins, 'Anecd. Med. Gr.,' p. 161.

[12] Galen, 'De Hipp. et Plat. Decr.,' iv, 4, tom. v, p. 383, l. 7, &c.

[13] Id., 'De Remed. Parab.,' iii, tom. xiv, p. 508, l. 10.

[14] 'Epid.,' iii, case 3, tome iii, p. 114, ll. 3, 9, ed. Littré.

[15] 'Comment. in Hippocr. "Epid. III,"' iii, 74, tom. xvii A, p. 757.

[16] See his 'Œcon. Hippocr.,' in ἀναυδία.

tioned by Liddell and Scott; but Ermerins, in his edition of Hippocrates (vol. i, p. 226), objects to it as too refined, and considers that the second word, *ἄφωνος*, is simply a gloss of the first, *ἄναυδος*, and ought therefore to be omitted from the text in both places. There seems, however, to be no authority whatever for this omission, which is not absolutely required by the sense; and the error (if it be one) must be of very ancient date, as the present text can be traced up to the time of Sabinus,[1] who was an older commentator than Galen, and lived in the first century after Christ.

ἄχηλος, a word not to be found in the Greek lexicons, is used by Galen,[2] as synonymous with *μονώνυχος*,[3] and applied to a class of quadrupeds without horns, and walking on single hoofs (*ὁπλαῖς μονοφυέσιν*), corresponding with Cuvier's family of the *Solipeda*. It is also synonymous with *μονόχηλος*, *ἀσχιδής*, and signifies *having solid hoofs*.

ἀχλύς is used sometimes in a general sense, and signifies merely *dimness of sight*, from whatever cause it may arise;[4] sometimes it is the result of superficial ulceration.[5] The word is found applied to the uterus in the common editions of Hippocrates,[6] but the text is undoubtedly corrupt, and has been altered by Ermerins accordingly. In Aretæus[7] the word is used metaphorically and poetically, and with an evident allusion to Homer, 'Il.,' v, 127.

ἀχλυόω, to cause dimness of sight.[8]

ἀχλυώδης is applied in the Hippocratic Collection to a person *affected with dimness of sight*,[9] or to the eye itself,[10] and to a wind,[11] or a fever,[12] *producing dimness of sight*, without in either case indicating or implying the precise cause of the affection. In Aretæus[13] Adams renders *ἀχλυώδης ὁ ἄνθρωπος* by "the patient becomes dark-coloured," but there seems to be no reason why the word should not bear the usual meaning in this passage. The Latin *caligare*, by which it is rendered in the Latin versions, will bear either sense.

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[1] See Galen, *ibid.*, p. 759.

[2] 'De Anat. Admin.,' iv, 8, tom. ii, p. 431, l. 1.

[3] *Ibid.*, vi, 8, p. 545, l. 10; p. 548, l. 8.

[4] Erotianus, 'Expos. Voc. Hippocr.' This appears to be the meaning of the word in Paulus Ægineta, i, 31, p. 4 B, l. ult., ed. Ald., copied from Oribasius, 'Synops.,' v, 27; p. 80, ed. H. Steph. The same expression, "dimness of sight," which is used by Adams in this place as the translation of *ἀχλύς* (vol. i, p. 40), is used by him in two other passages as the rendering of *ἀμβλυωπία*, iii, 22, vol. i, pp. 419, 421.

[5] Aëtius, vii, 27, p. 127, l. 48, ed. Ald.; Pseudo-Galen, 'Introd.,' c. 16, tom. xiv, p. 774, l. 8; Leo, 'Consp. Medic.,' iii, 27, in Ermerins, 'Anecd. Med. Gr.,' p. 148.

[6] 'De Morb. Mul.,' ii, § 172, tome viii, p. 352, ed. Littré, vol. ii, p. 762, ed. Erm.

[7] 'De Caus. Acut.,' ii, 4, p. 33, l. 2, ed. Adams.

[8] Aretæus, 'De Cur. Chron.,' i, 3, p. 214, l. 5, ed. Adams.

[9] 'Coac. Praenot.,' § 36, tome v, p. 594, ed. Littré.

[10] *Ibid.*, § 221; 'Prorrhet.,' i, § 46, tome v, pp. 632, 522. So too in Galen, 'Comment. in Hippocr. "Prorrhet I,"' ii, 45, tom. xvi, p. 609, l. 16; p. 610, ll. 1, 8; and in Aretæus, 'Caus. Chron.,' ii, 13, p. 127, l. 21, ed. Adams.

[11] 'Aphor.,' iii, 5, tome iv. p. 488.

[12] 'Prorrhet.,' i, § 74, tome v, p. 528. In this passage Ermerins (vol. i, p. 16) has without authority or necessity altered the text, though the same words are found in Galen, *loco cit.*, p. 663.

[13] 'De Cur. Acut.,' ii, 4, p. 187, l. 8; p. 436, l. ult.

ἄχολος, *without bile*, probably almost always applied to the fæces;[1] sometimes *not producing bile*, as ἄχολον γάλα, applied to ass's milk.[2]

ἄχροια signifies *want of colour*, or *a bad colour*, opposed to ἐξέρυθρος χρώς,[3] or ἔυχροια;[4] but sometimes bears either or both senses indiscriminately.[5] In a difficult passage of Aretæus occurs the phrase προσώπου ἐρύθημα ξὺν ἀχροίῃ, which is literally translated by Adams,[6] "redness of countenance, along with paleness;" but these words are as puzzling as the Greek text. Ermerins changes the word into ἐυχροίῃ (p. 47, l. 9), and renders the passage, "vultus ruber et bene coloratus" (p. 307, l. 16); but there is no authority for the alteration, and "redness of countenance with a good (*i. e.* a *red*) colour" looks like tautology. Perhaps the word may signify *irregularity or inequality of colour*, or, perhaps, *occasional or partial paleness* is to be understood; but neither of these explanations is perfectly satisfactory.[7]

ἀχροιέω, or ἀχροέω, to become ἄχροιος or ἄχροος.[8]

ἄχροιος, or ἄχροος, ἄχρους, *colourless*, *limpid*, applied to semen;[9] *colourless*, *pale*, *ill-coloured*, applied to the complexion,[10] nearly synonymous with λείφαιμος, but a stronger word.[11]

ἄχωρ,[12] sometimes called a ἕλκος, as if applied to a single pustule,[13] sometimes a πάθος, as signifying a collection of pustules.[14] It differs from κηρίον, *favus*, (which it closely resembles,) chiefly as consisting of smaller pustules; both words being connected with *porrigo*,[15] and signifying an affection of the scalp. The origin of the word is quite uncertain; the derivation from α and χώρα ("quod non amplum locum occupet"), is attributed to Galen,[1] but no reference is given, and it is difficult to imagine that Galen should ever have proposed anything so absurd. The author of the spurious treatise 'De Remediis Parabilibus'[2] derives the word from ἰχώρ, on account of the *ichorous* discharge from the pustules; but this etymology is not much more probable than the former, and others that have been proposed are not more satisfactory.

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[1] Aretæus, 'De Caus. Chron.,' i, 16; ii, 7, 9, p. 122, l. 6; p. 150, l. 8; p. 155, l. 12, ed. Kühn.

[2] Id., 'De Cur. Chron.,' i, 13, p. 326, l. ult.

[3] Hippocrates, 'Epid. VI,' vi, 7, tome v, pp. 327, 328, ed. Littré.

[4] Aretæus, 'De Cur. Chron.,' ii, 13, p. 240, l. ult., ed. Adams.

[5] As probably in Aretæus, 'De Caus. Chron.,' i, 8, p. 67, l. 12.

[6] 'De Caus. Acut.,' ii, 8, p. 39, l. 24; p. 281, l. 2.

[7] See Petit's Commentary on the place, p. 435, ed. Kühn.

[8] Hippocrates, 'De Fract.,' § 25, tome iii, p. 498, l. penult., ed. Littré.

[9] Aretæus, 'De Caus. Chron.,' ii, 5, p. 103, l. antep., ed. Adams.

[10] Hippocrates, 'Prorrhet.,' i, 71, tome v, p. 528, l. 5, ed. Littré; Aretæus, 'De Caus. Chron.,' i, 6, p. 62, l. 9.

[11] Galen, 'Comment. in Hippocr. "De Vict. Acut.,"' iv, 97, tom. xv, p. 904, ll. 4, 15, 17.

[12] This, according to Liddell and Scott, is the old and genuine form of the word, though, probably, in all the existing editions of the Greek medical authors it is written ἀχώρ.

[13] Galen, 'De Tumor. praeter Nat.,' c. 15, tom. vii, p. 728, l. 8. Hence, perhaps, the use of the word in the plural, ἄχωρες, Alexander Trallianus, i, 9, pp. 14, 15, ed. Basil.

[14] Id., 'De Comp. Med. sec. Loc.,' i, 8, tom. xii, p. 463, l. penult.

[15] See Bateman's 'Cutaneous Diseases,' "Porrigo," and "Definitions." Both words are found in the Hippocratic Collection, and both are translated "favus" by Littré. 'De Affect.,' § 35, tome vi, p. 246; "Prorrhet.,' ii, 11; 'De Alim.,' § 20, tom. ix, pp. 32, 104.

ἅψις, literally *a touching*. The expression ἅψις φρενῶν occurs twice in the Hippocratic Collection,[3] and is explained by Galen,[4] to signify *παραφροσύνη, delirium*. The similar phrase ἅπτεσθαι φρενῶν is also found.[5] Aretæus, who is fond of Hippocratic words and phrases, uses the expression twice. In one passage,[6] manifestly in the sense of *delirium;* and possibly in the other also,[7] though in this latter the word φρενῶν is generally taken to signify the *diaphragm*.

ἀψυχέω occurs in the Hippocratic Collection,[8] and is explained by Erotianus[9] and Galen[10] to signify λειποθυμέω, *to swoon:*—it probably never means *to be ἄψυχος*, in the sense of *lifeless*.

ἀψυχία occurs in the Hippocratic Collection,[11] and is explained by Galen to signify λειποθυμία,[12] or λειποψυχία,[13] *swooning*. Sometimes it means merely *faintness*,[14] sometimes *loss of spirits*,[15] but probably never *lifelessness*. The word is sometimes found in the plural.[16] The word ἀψυξίην is found in Kühn's Aretæus,[17] but no such word is recognised by Liddell and Scott, and Ermerins and Adams are no doubt right in reading ἀψυχίην.[18] The word *apopsychia* is found in some medical dictionaries, but it is of doubtful authority, and rests only on one passage of Galen,[19] where the reading is undoubtedly corrupt, but where he seems to say that Dioscorides, in his edition of Hippocrates,[1] read ἀποψυχίη for ἀψυχίη.

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[1] Scapulae 'Lex. Graeco-Lat.'

[2] i, 2, Galen, tom. xiv, p. 323, l. 11.

[3] 'De Vict. Rat. in Morb. Ac.,' § 17, tome ii, p. 360, l. 10, ed. Littré; 'De Morb. Mul.,' i, 63, tome viii, p. 128, l. antep.

[4] 'Comment. in Hippocr. "De Vict. Rat. in Morb. Ac.,"' iii, 37, tom. xv, p. 701, l. 7; 'Comment. in Hippocr. "Prognost.,"' ii, 70, tom. xviii B, p. 223, l. 8.

[5] See Foesii 'Œcon. Hippocr.'

[6] 'De Cur. Morb. Acut.,' ii, 3, p. 183, l. 9, ed. Adams.

[7] *Ibid.*, i, 1, p. 136, l. ult.

[8] 'De Morb.,' ii, 5, tome vii, p. 12, l. 33, ed. Littré.

[9] 'Lex. Hippocr.,' p. 47, ed. Klein.

[10] 'Gloss. Hippocr.,' tom. xix, p. 87.

[11] 'De Vict. Acut.,' §§ 9, 10, tome ii, p. 440, l. antep.; p. 444, l. 3, ed. Littré.

[12] 'Comment. in Hippocr. "De Vict. Acut.,"' iv, 54, tom. xv, p. 828, ll. 1, 12.

[13] 'Comment. in Hippocr. "Prorrhet. I,"' iii, 114, tom. xvi, p. 756, l. 7; 'Gloss. Hippocr.,' tom. xix, p. 87.

[14] Hippocr., 'De Vet. Med.,' § 10, tome i, p. 595, l. 15, ed. Littré.

[15] Aretæus, 'De Sign. Morb. Chron.,' ii, 1, p. 94, l. 6, ed. Adams.

[16] Hippocrates, 'De Vict. Acut.,' § 10, tome v, p. 442, l. 8, ed Littré; 'De Morb. Mul.,' § 63, tome viii, p. 128, l. 18.

[17] 'De Sign. Morb. Chron.,' i, 7, p. 85, l. 13.

[18] Kühn's reading is not a typographical error, as may be seen from his 'Opusc. Acad. Med. et Philol.,' vol. ii, p. 357.

[19] *Loco cit.*, tom. xvi, p. 756, l. 8.

ἄψυχοι, *lifeless*,[2] *spiritless*,[3] probably never *in a swoon*.

[1] 'Prorrhet.,' i, 113, tom. v, p. 546, ed. Littré.

[2] Galen, 'De Temper.,' i, 8, tom. i, p. 555, l. 3; Aretæus, 'De Cur. Morb. Acut.,' i, 4, p. 152, l. penult., ed. Adams; 'De Sign. Morb. Chron.,' ii, 5, p. 103, l. 21; Soranus, 'De Morb. Mul.,' c. 116, p. 267, l. 7.

[3] Aretæus, *loco cit.*, p. 103, l. ult.; c. 6, p. 105, l. 23.

'DIXON'S GUIDE TO THE PRACTICAL STUDY OF DISEASES OF THE EYE,' &c.

We very willingly give insertion to the following note, regarding certain observations which appeared in our previous number regarding Mr. Dixon's recent work on diseases of the eye.

To the Editor of the 'British and Foreign Medico-Chirurgical Review.'

Sir,—In reviewing the new edition of my book on diseases of the eye (July number, p. 77), your critic observes that a passage relating to the employment of atropine in the acute stage of iritis, which appeared in the second edition (1859), has been wholly suppressed in the third, without any reference to the subject having been made, or any reason assigned for the omission. This, he says, is "very remarkable," and "not praiseworthy."

Now, as this is a matter which, although of little interest to others, involves my reputation for candour and fair dealing, perhaps you will allow me a small space in your next number to explain that the omission of which your critic complains, was the result of a mere oversight on my part. I had cancelled the passage at pp. 139-40 of the second edition, with the object of replacing it by a paragraph embodying my present views, as based upon a more extended experience of the use of atropine. I wrote out such a paragraph as a rough note, but through mere carelessness it was not copied into the MS. which was sent to press; and it was not until the whole edition had been printed off that I became aware of my act of negligence.

Your obedient Servant,
James Dixon.

Portman Square,
Aug. 4th, 1866.

BOOKS, PAMPHLETS, &c., RECEIVED FOR REVIEW.

The Physiological Anatomy and Physiology of Man. By R. B. Todd, W. Bowman, and L. S. Beale. A new Edition by the last-named Author. 1866. Part I. pp. 155.

Braithwaite's Retrospect of Medicine. Vol. LIII. January—June, 1866. London, Simpkin, Marshall, and Co. pp. 420.

On the Railway and other Injuries of the Nervous System. By J. Eric Erichsen, F.R.C.P., Professor of Surgery, &c., University College. London, Walton and Maberly. 1866. pp. 144.

Tape-Worms (Human Entozoa); their Sources, Nature, and Treatment. By T. S. Cobbold, M.D., F.R.S., Lecturer at Middlesex Hospital. London, Longman and Co. 1866. pp. 83.

Gleet; its Pathology and Treatment; with a Memoir on Treatment of Stricture of Urethra by Subcutaneous Division. By Henry Dick, M.D. Second Edition. Hardwicke. 1866. pp. 113.

Clinical Memoirs on the Diseases of Women. By M. G. Bernutz and M. E. Goupil. In two volumes. Vol. I. pp. 276. (Translated and Edited for the New Sydenham Society, by Alfred Meadows, M.D.)

The Half-Yearly Abstract of the Medical Sciences. Vol. XLIII. January—June, 1866. London, Churchill and Sons. 1866. pp. 371.

The English and their Origin. A Prologue to Authentic English History. By Luke O. Pike, M.A., Barrister-at-Law. London, Longman and Co. 1866. pp. 267.

A Manual of Practical Hygiene; prepared especially for the Use (in the Medical Service) of the Army. By E. A. Parkes, M.D., F.R.S. Second Edition. London, Churchill and Sons. 1866. pp. 624.

Sore Throat: its Nature, Varieties, and Treatment; including the Use of the Laryngoscope as an Aid of Diagnosis. By M. Prosser James, M.D., Senior Physician to the City Dispensary. Second Edition. London, Churchill and Sons. 1866. pp. 155.

Handbook of Natural Philosophy. By D. Lardner, D.C.L. (Seventh Thousand.) Edited by G. C. Foster. London, Walton and Maberly. 1866. pp. 942.

Climate of the Swiss Alps and of the Peruvian Andes Compared. By A. Smith, M.D. (Reprint from Dublin 'Quarterly Journal,' May, 1866.) Pamphlet.

On the Chemical Pathology of the Brain. By A. Addison, L.R.C.P. (Reprint from 'Journal of Mental Science,' July, 1866.)

Vivisection: is it Necessary or Justifiable? Two Prize Essays, published by the Royal Society for Prevention of Cruelty to Animals. By G. Fleming, and W. O. Markham, M.D. (Pamphlet.) London, Hardwicke.

Memoir of John Stearne, M.D., &c., Founder and First President of the College of Physicians, Dublin. By T. W. Belcher, M.D. 1865. (Pamphlet.)

Leprosy. (Reprint from 'Indian Medical Gazette.') By C. Macnamara, Surgeon. Calcutta. (Pamphlet.) 1866.

Defects of Sight and Hearing, &c., &c. By T. Wharton Jones, F.R.S. London, Churchill and Sons. 1866. pp. 168.

A Winter in Paris: being a few Experiences and Observations of French Medical and Sanitary Matters during the session, 1865—6. By F. Simms, M.B. London, Churchill and Sons. 1866. pp. 151.

On Cholera: its Nature and Treatment; being the Debate in the Harleian Medical Society of London. Edited by Dr. C. Drysdale, Hon. Secretary. (Pamphlet.) Hardwicke. 1866.

Clinical Lectures on Diseases of the Heart, delivered at the Mater Misericordiæ Hospital. By T. Haden. (Mitral Obstruction.) (Pamphlet.) Dublin.

A Manual of the Operations of Surgery for the Use of Senior Students, House Surgeons, and Junior Practitioners. By J. Bell, Lecturer on Surgery, &c., Edinburgh and London. 1866. pp. 267.

The Antidotal Treatment of the Epidemic Cholera; with Directions, &c., for the Prevention of the Disease. By J. Parkin, M.D., late Medical Inspector for Cholera in the West Indies. London, Churchill and Sons. 1866. pp. 321.

Army Hygiene. By C. A. Gordon, M.D., C.B., Deputy Inspector-General of Hospitals, &c. 1866. London and Calcutta. 1866. pp. 532.

A Simple Explanation of Cholera, and a Rational Mode of Treating it. By Yod, M.D. (Pamphlet.) Renshaw.

An Essay on Yellow Fever, comprising the History of that Disease as it appeared in the Island of Antigua in 1835, 1839, and 1842, &c. By T. Nicholson, M.D., late Physician to the Public Hospital, Antigua. Second Edition. London, Churchill and Sons. 1866. pp. 62.

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On the Mode of Action of Strychnia. By A. J. Spence, M.D., Assistant-Physician, Royal Edinburgh. Asylum. (Read before the Med. Chir. Soc., May 14, 1866. Pamphlet.)

Medical Diagnosis, with Special Reference to Practical Medicine. By J. M. Da Costa, M.D., Lecturer on Clinical Medicine, Physician to the Pennsylvania Hospital, &c. Second Edition. Philadelphia, Lippincott and Co. 1866. pp. 784.

On Epidemic Diarrhœa and Cholera: their Nature and Treatment. By G. Johnson, M.D., Physician to King's College, &c. (Pamphlet.) London, Hardwicke.

On the Rational Employment of Mercury in the Treatment of Syphilis. By Dr. C. Meredyth. (Pamphlet.) London, Hardwicke.

On the Treatment of Asiatic Cholera. By A. Billing, M.D. New Edition. (Pamphlet.) London, Churchill and Sons.

The Medical Acts and the Proposed "Medical Acts Amendment Bill." London, Mitchell and Co.

Diabetes: its Various Forms and Different Treatments. By G. Harley, M.D., F.R.S., Professor in University College, &c. London, Walton and Maberly. 1866. pp. 74.

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NOTICE TO READERS.

The Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him, as also Inaugural Lectures, Dissertations for Theses, Medical and Scientific Addresses, &c.

INDEX TO VOL. XXXVIII.

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

PAGE
ACLAND'S Harveian oration . 353
Aconite detection, on . 452
Adams on aconite detection . 452
Adversaria medico-philologica . 551
Aiton on cholera . 129
Allaire on urethritis . 264
Allbutt on medicine of Greeks . 483
American prisoners of war . 460
Anæsthesia, Ellis on . 479
Anæsthetic agents, Wecker on . 272
Anderson on cholera and yellow fever . 129
Anderson McCall on skin parasites 225
Aneurysms, cirsoid, on . 269
Anthrax, Larghi on . 272
Anthropology, Blumenbach on . 465
Arnott's physics . 436

Baly and Gull on cholera . 129
Barclay on gout, rheumatism, and heart disease . 477
Barnes' report on midwifery . 277
Barwell on club-foot . 23
Baeza on Cæsarian section . 279
Bérenge Féraud on wounds . 268
Bigg on orthopraxy . 444
Blondel et Ser on Hospitals . 370
Blumenbach on anthropology . 465
——— memoirs of . 465
Bonnafont on cholera . 130
——— on ear diseases . 29
Bourgignon on cattle plague . 307

PAGE
Brain, sclerosis of . 259
Brande's dictionary of science and art . 445
Briand's legal medicine . 2
Bright's disease, Stewart, G., on . 196
Bristowe on hospitals . 370
Byford on uterine disease . 434
Byrne (J. A.) on puerperal abscess 281
Byrne (B. M.) on cholera . 417

Cæsarian section, Baeza on . 279
Carbolic acid, on . 433
Casper's forensic medicine . 2
Cataract, Kruse on . 261
Cattle plague, works on . 307
Chambers on climate of Italy . 170
Chaudé's legal medicine . 2
Chatto's surgical chronicle . 268
Chemistry, medico-legal . 2
Chemistry, Odling's . 432
Chloroform, Sansom on . 47
Cholera, authorities on . 129, 417
Cholora, Pacini on . 167
Chorion, cystic disease of . 264
Club foot, Barwell on . 23
Cocteau on cirsoid aneurism . 269
Comparative anatomy, Van der Hoeven on . 162
Curling on the testis, &c. . 407

Daldy on diseased heart . 429
Davy (John) on butcher's meat . 170

PAGE
Demarquay on hernia . . . 271
De Morgan, Campbell, on excision of spinal accessory nerve . . 218
Dermatology, M'Call Anderson on 476
Diabetic kidneys, Krause on . 262
Dictionary of science, &c. . . 445
Digitaline, tests for . . . 452
Dixon on eye-diseases . . . 62
Dixon's 'Diseases of Eye,' letter on 560
Donaldson on cholera . . . 130
Donders on rapidity of thought . 168
Duncan (M.) on pelvic effusions . 281
——— (J. F.) on responsibility of insane 434

Ear-diseases, works on . . 29
Ebstein on gastric mucous membrane 263
Echinococci, Rasmussen on . . 285
Ellis (R.) on anæsthesia . . 474
Embolia, puerperal, on . . 280
Emmet on ruptured uterus . 277

Fagge on poison detection . . 452
Falret on insanity . . . 441
Faure on varices 270
Faye on uterine disease . . 465
Fleming on cattle disease . . 307
Fœtal intermittent fever . . 278
Food, Ireland on . . . 456
—— Panum on 83
Fox (Tilbury) on cholera . . 130

Galezowski on optic-nerve changes 63
Gamgee (J.) on cattle plague . 307
Gibbons on carbolic acid . . . 483
Goodeve on cholera . . . 129
Gordon on dislocation of humerus 498
Gordon on puerperal tetanus . 282
Gout, rheumatism, and heart disease, Barclay on . . . 477

PAGE
Graves on cholera . . . 129
"Graves' disease," Moore on . 431
Greenhill's Adversaria Medico-Philologica 551
Greeks, medicine of . . . 483

Harveian oration, Acland's . . 353
Hasselt on toxicology . . . 1
Helminthology, Danish works on 285
Hereditary disease, Sedgwick on .
Hernia, umbilical, Démarquay on . 271
Hicks (B.) on cystic chorion . 264
Holmes on hospitals . . . 370
Howe on cholera 219
Hospital, on admissions at . . 471
——— construction, works on . 370
——— London, Reports . . 103
——— St. Bartholomew's, Reports 103
Hospitals, Peacock on . . . 236
Hubbard on fœtal intermittent fever 278
Humerus dislocation, Gordon on . 498
Hunter (John, M.D.) on varieties of man 465
Husemann on toxicology . . 1
Husson on hospitals 370
Hypertrophy of gastric mucous membrane 263

Insane, responsibility of . . 434
Insanity, Falret on . . . 441
Ireland on food 456
Italy, Chambers on . . . 170

Johnson (G.) on cholera . . 129
Jones (Bence) on animal quinine . 174
——— (Wharton) on ophthalmic medicine 62
Jurisprudence, medical works on . 1

Krabbe on hydatids . . . 285
Kramer on ear-diseases . . 29
Krause on diabetic kidneys . . 262

PAGE

Kruse on cataract . . . 261

Kuhne on motor fibres . . . 256

Lacerated vagina, M'Clintock on . 278

Larghi on anthrax . . . 272

Lawrence on optical eye defects . 63

——— on ophthalmic surgery . 63

Layard on cattle disease . . 307

Lee (H.) on diseases of veins, &c. 363

Life assurance, Robertson on . 433

Materia Medica, Semple's report on

M'Call (Anderson) on dermatology 476

M'Clintock on lacerated vagina . 278

Macnamara on eye diseases . . 63

Macpherson on cholera . . 417

Malformations of generative organs 277

Marsh fever, changes of blood in . 259

Medicine, Webb's report on . . 539

Meigs on blood changes . . 259

Mental diseases, Sankey on . . 346

Mesnet on cholera . . . 129

Micrological Chronicle, Streatfeild's 256

Midwifery Chronicle, Barnes' . 277

Molluscum contagiosum . . 264

Moore (C. F.) on cholera . . 130

Moore (W.) on Graves' disease . 431

More on dilatation of perinæum . 278

Morehead on cholera . . . 129

Motor fibres, Kuhne on . . 256

Munro on cholera 417

Mushet (W. B.) on nervous apoplexy 177

Nervous apoplexy, Mushet on . 177

Nightingale (F.) on hospitals . 370

Nitrite of amyl, Richardson on . 450

Obstetrical Society's transactions . 397

Odling's practical chemistry . 432

Ogle (John W.) on cholera . . 129

Onychomychosis, Purser on . . 267

Ophthalmic medicine, works on . 62

PAGE

Orthopraxy, Bigg on . . . 444

Otto on poisons 2

Ovaries, Ritchie on . . . 439

Pacini on cholera 167

Panum on food 83

Parasites of skin, M'Call Anderson on 225

Parkes on hygiène . . . 370

Pathology, Webb's report on . 539

Peacock on German hospitals . 236

Pellarin on cholera . . . 130

Pelvic effusions, puerperal . . 281

Penny on aconite detection . . 452

Perinæum, dilatation of . . 278

Phthisis, Pollock on . . . 93

——— Ringer on . . . 93

Physics, Arnott on . . . 436

Poisons, Otto on 2

——— Taylor on 1

——— Virchow on 1

Pollock on phthisis . . . 93

Pozzi on hospitals . . . 370

Prisoners of war, American . . 460

Puerperal pulmonary abscess . 281

Purser on onychomychosis . . 267

Quinine, animal, Bence Jones on . 174

Rasmussen on echinocci . . 285

Retina, Swan on 63

Reynolds' system of medicine . 43

Richardson on nitrite of amyl . 450

Ritter on puerperal embolia . . 280

Rinderpest, on blood in . . 259

Ritchie on the ovaries . . . 439

Roberts on urinary diseases . . 301

Robertson on life assurance . . 433

Rumsey on state medicine . . 438

Ruptured inverted uterus, on . 277

Salisbury on spleen . . . 258

Sankey on mental diseases . . 346

Sansom on chloroform . . . 474

Schauenstein's forensic medicine . 2

PAGE

Scheffler's physiological optics . 63
Schneider's medico-legal chemistry 2
Schroder on childbed temperature 281
Sedgwick on cholera . . . 417
——— on hereditary disease . 501
Semple's Materia Medica report . 524
Smith (A.) on blood in rinderpest 259
Solly's surgical experiences . . 37
Spinal accessory nerve, excision of 218
Spitta on cholera . . . 130
Spleen, Salisbury on . . . 258
Stanski on contagion . . . 130
State-medicine, Rumsey on . . 438
Stephenson on poison-tests . . 452
Stewart, (G.) on Bright's disease . 196
Story on cholera 130
Streatfeild's micrological report . 256
Surgery, Chatto's Chronicle of . 268
Swan on the retina . . . 63

Taylor on medical jurisprudence . 1
——— on poisons . . . 1
Teeth, Waldeyer on . . . 256
Temperature of childbed . 280, 281
———, human, Ringer on . 93
Testis, Curling on . . . 407
Tetanus, puerperal . . . 282
Thörl on thyroid gland disease . 265
Thought, Donders on rapidity of . 168
Thyroïd, cysto-sarcoma of . . 265
Toxicology, works on . . . 1
Tracy on uterine tumours . . 278
Transactions of Obstetrical Society 397

PAGE

Trench on health of Liverpool . 480
Troeltsch on ear-diseases . . 29
Tuberculosis, works on . . 93
Tumours, fibrous, of uterus . . 278
Turner on malformations . . 277

United States' Sanitary Commission 447
Urethritis, Allaire on . . . 269
Urinary diseases, Roberts on . 301
Uterine disease, Byford on . . 434
Uterine malformations, Fagge on . 465
Utterhoeven on hospitals . . 370

Van der Hoeven on zoology, &c. . 162
Varices, Faure on . . . 270
Veins, diseases of, Lee on . . 363
Virchow on poisoning . . 1
——— on molluscum contagiosum 264

Watson on cholera 129
Webb's report on pathology and medicine 539
Wecker on ophthalmology . . 62
——— on anæsthetic agents . 272
Waldeyer on teeth . . . 256
Wolf on temperature of childbed . 280
Woodhouse on carbolic acid . 433
Wounds treated by ventilation . 268

Zenker on sclerosis of brain . . 259
Zoological philosophy, Van der Hoeven on 162

END OF VOL. XXXVIII.